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65384

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CONTENTS.

On Tragedy and the Interest in Tragical Representations: An Essay. By the Rev. George Walker, F. R. S. *and Professor of Theology in the New College, Manchester.* page 319

Experiments and Observations to determine whether the quantity of Rain and Dew is equal to the quantity of Water carried off by the Rivers and raised by Evaporation; with an Enquiry into the Origin of Springs. By Mr. John Dalton .... p. 346

Experiments and Observations on the power of Fluids to conduct Heat; with reference to Count Rumford's *Seventh Essay on the same subject. By* Mr. John Dalton .... p. 373

Experiments on the Velocity of Air issuing out of a Vessel in different circumstances; with the Description of an Instrument to measure the Force of the Blast in Bellows, &c. By Mr. Banks, *Lecturer in Natural Philosophy. —Communicated by* Mr. Dalton .... p. 398

Essay on the Beautiful in the Human Form; and Enquiry whether the Grecian Statues present the most perfect Beauty of Form, that we at present have any Acquaintance with.—Communicated to the Society from a Correspondent, *through the* Rev. George Walker. p. 407

A Defence of Learning and the Arts, against some charges of Rousseau. *In two Essays. By the* Rev. George Walker, F. R. S. .... p. 438

Observations on the Nervous Systems of different Animals; on Original Defects in the Nervous System of the Human Species and their Influence on Sensation and Voluntary Motion. By John Hull, M. D. .... p. 475

Experiments and Observations on the Heat and Cold produced by the Mechanical condensation and rarefaction of Air. By Mr. John Dalton .... p. 515

Account of some Antiques lately found in the River Ribble. By Mr. Thomas Barritt. p. 527

Experimental Essays, on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different temperatures, both in a Torricellian Vacuum *and in Air; on Evaporation; and on the Expansion of Gases by Heat. By* Mr. John Dalton .... p. 535

A Review of some Experiments, which have been supposed to disprove the Materiality of Heat. By Mr. William Henry .... p. 603

An Investigation of the Method whereby Men judge, by the Ear, of the Position of Sonorous Bodies relative to their own Persons. By Mr. John Gough.—*Communicated by* Dr. Holme .... p. 622

On the Theory of Compound Sounds. By Mr. John Gough.—*Communicated by* Dr. Holme .... p. 653

Meteorological Observations, made at Manchester. By Mr. John Dalton. .... p. 666

APPENDIX. I. *Explanation of a Roman Inscription, found in Castle-field, Manchester. By* Mr. Thomas Barritt: *with a Note on the same subject by* Dr. Holme .... p. 675

II. *Note to* Mr. W. Henry's *Paper on Heat* .... p. 679

ERRATA.

Page 31, line 24, for *yellow* read *blue.*
—— 38, add to the note, *Cahier de Fevrier,* 1779.
—— 355, line 2, for 600 read 6000.
—— 357, line 3, for 150 read 154—for 182 read 189.
—— 600, line 5 of Note, for *on,* read *as.*
—— 607, for *amse,* read *same.*

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ON TRAGEDY & THE INTEREST IN TRAGICAL REPRESENTATIONS.

AN ESSAY.

BY THE REVEREND GEO. WALKER, F. R. S:
And Profeſſor of Theology in the New College, Mancheſter.

The propenſity which human nature has, in every age and nation, diſcovered for ſpectacles and repreſentations of a tragic kind, though it be univerſally confeſſed, that the ſenſations and paſſions excited thereby are in their nature painful, and often exquiſitely ſo, is at firſt view ſo ſingular and contradictory a phenomenon, as could not fail to draw the attention of moraliſts and philoſophers; and challenge all their ingenuity to reconcile ſo irregular a trait of the human character, with the most approved likeness of the human mind. There are, indeed, some examples of this propensity so rude, uncivilized, and inhuman, as mock all efforts of ingenuity to reduce them to a consistent and agreeable system. Such were the exhibitions of gladiators, among the Romans; the tournaments and justs of gothic chivalry; such are the bull-fights of the Spaniards; the combats with the broad sword; the bull-baitings, cock-fights, and Shrove-tide amusements of our own nation; together with the horrid jollity of the North American tribes, exulting over the tortures of their ill-fated prisoners.

Most or all of these national reproaches are, in a greater or less degree, the offspring of a rude military genius, and savage heroism; which, by an early familiarity with the excesses and cruelties of war, let loose in all its wildness, have triumphed over nature, over the kinder dictates of a general humanity. In these, the pleasure of the spectators, unnatural as it is, is pure and unmixed; by whatever means they have subdued their minds to the capacity of this pleasure, when once the relish is acquired, their continued propensity to such scenes is perfectly natural, as it is not combated by any feeling of sympathetic pain during the exhibition. This is clearly attested of the Roman people, by the uniform accounts of their own historians: foreign nations ascribed their hardineſs in war to this familiarity with blood and death in the amphitheatre; and a Syrian monarch, ambitious, at any rate, to rival the Roman grandeur, hoped to render his effeminate Asiatics equally intrepid and unappalled amidst the horrors of battle, by introducing the same sanguinary entertainments in peace. It is a proof that humanity had little interest in the fatal consequences of the Gothic tournaments, and bull-fights of the Spaniards, when that sex, whom compassion and an abhorrence from spectacles of blood may be supposed the last to forsake, was admitted to the most conspicuous seats; as if to gratify them were the chief object of the entertainment. All is mad mirth, and drunken joy, with an American village, while their captive is wasting under their protracted tortures; compassion, or even indifference, would vitiate the festival. Humanity may have repelled from, but never invited a single guest to the cruel entertainments of our own nation. It is to that polished humanity, which a cultivated philosophy and a purer religion have introduced amongst us, that we owe the disrepute into which these vulgar jollities have at length happily fallen.

Such representations are, therefore, utterly dissimilar, in their effects upon the heart, to the representations of tragedy and romance. Humanity renounces the one, but welcomes the other. In those a brutal joy reigns triumphant;

in these—if there be a joy, it is of a singular kind; it wears all the dress of sorrow; and the heart feels that there is a pain more than proportioned to the joy. It is surely, therefore, unphilosophical, to reduce, under one class, propensities which are of so different a cast and influence; nor can that ingenious French critic, the Abbè du Bos, be justified, in deriving them, without distinction, from one common source in the human mind.

But whence then is derived, and how are we to account for, this strange intermixture of pain and a something like joy, excited, in the same instant, by the same object, each apparently dependant on each other, and yet not blended together in one undistinguished mass.—Before I attempt the solution of this singular, but universal character of man, it may not be amiss to take a brief view of some of the most celebrated theories on this subject, and the rather, as the examination of these may lead to the true solution.

The French critic just mentioned, the Abbè du Bos, whose reflections on poetry, painting, and music, form a very entertaining work, refers the solution of this difficulty to that aversion which we have to indolence; and in consequonce to the delight we feel in having our most active and lively passions roused.

This account is striking and bold, as it derives a great deal from one simple and uniform principle. If, indeed, there be such a principle in human nature, exactly as he represents it, original and independant; and if it be adequate to the effect which he ascribes to it; and if the mind be sensible of such a reference in the instant of its most interesting emotions; and if the inclination to be thus moved, be proportioned to the force of this supposed principle; we could not wish for a more satisfactory solution, for one which more happily applied itself to the whole subject in question.

Another ingenious Frenchman, Monsieur Fontenelle, so well known to the literary world by his Dialogues, History of Oracles, and Plurality of Worlds, in some reflections on the subject of poetry, has hazarded the following fine-spun theory:—Pleasure and pain, says he, like many other extremes, approach, and, at a certain point, pass into each other. Pleasure, pushed too far, becomes pain; and the movement of pain, a little moderated, becomes pleasure.—He is obliged to adopt into his system the fundamental principle of Du Bos; for the heart, he says, loves to be moved, and therefore objects, which are melancholy, and even disastrous and sorrowful, are adapted to it; provided that they are softened by some circumstance. This softning circumstance, according to him, is the comfortable reflection, that the whole is but a fiction;

without which, the spectacle would be painful beyond the degree, in which it is capable of passing into pleasure.

This is the spirit of Fontenelle's theory; a theory so exceedingly refined, that we hardly know how to lay hold of it. It does not present us with any thing analogous to the real feelings of the heart; and is, indeed, contradictory to the very nature of things. Pleasure and pain, as simple sensations, have no intercourse with each other; though the transitions from the one to the other may be exceedingly quick, and may have their origin from the same external objects. For as objects are of a mixed character, the sensations may be mixed also; and in some, the painful circumstance, after a certain interval, may disappear, and vice versâ. But where the characters of the painful and the pleasant continue undiminished, the sensations which correspond to them will continue also; and each, as the causes of them are alternately contemplated, be separately excited; or that, which is the balance of the separate sensations will remain.

If, as Fontenelle asserts, pain can of itself pass into pleasure, and without any additional cause, and it be in the moment of the transition that the pleasureable sensation presents itself, it will be exceedingly difficult to determine, according to this theory, what the predominant sensation will be. If the painful sensation be then evanescent, the pleasureable one ought to be nearly unmixed; if the painful one be then at its height, the senſation of pleasure must be hardly perceivable, and cannot, methinks, account for the interest which we have in the representation. For the feelings, during a tragical representation, are not of this dubious and indeterminate character; the pain and the pleasure, if we must give the denomination of pleasure to the interest which we have in the spectacle, are distinct, and at the same moment are each highly exquisite. There is, in truth, no passing of the one into the other.

It is a farther objection to the theory of Fontenelle, that he has assigned no proper source of pleasure, which can give its complexion to the pain; the circumstance which he has noticed is merely an alleviation, and can only account for a diminution of the pain. This circumstance of the whole being but a fiction, has exceedingly little, if any, influence in softening and alleviating our painful sensation; though it be true, that if it were a spectacle of real misery, we should be repelled from approaching it at all. But it is also true, that the more the fiction is kept out of view, the more perfect is the art of the poet, and the more perfect the effect of the imitation upon the mind of the spectator; whose interest rises to its greatest height, when, by a kind of divine power, he is carried entirely out of the consideration of self, and contemplates nothing but the misery, as if it were real, and enters into it with all the glow of natural feeling. The solution, therefore, must be sought for in some other principle than the whimsical conceit of a middle something, between pleasure and pain, founded on the cold reflection, that the whole is a delusion. It may account, in some degree, for the phlegmatic dialogues of a French tragedy-maker; and for the dubious sensation, the middle something between pleasure and pain, which they excite; but will never unfold the feelings which the magic genius of Shakespeare stirs up in the soul.

The selfish system in morals, which extracts a joy out of a painful scene, from the groveling consideration, that, whatever sufferings are exhibited to our view, we are ourselves in a state of perfect security, is so grossly false, that a moment's consideration shakes it off with indignation, and leaves it to the sordid soul, which first conceived the idea. This would suppose that suffering and distress were in themselves a grateful spectacle, if they affect not ourselves; that there was a real malignity in the human heart, and that it recurred to them as to a feast. This is a horrid untruth; such spectacles are, in their nature, painful; and all that the consideration of our own security can effect, will be only to render them less painful; but of itself can give us no interest in them, nor render them at all attracting to us.

The celebrated David Hume has offered a more plausible theory; or rather, has added to the systems of Du Bos and Fontenelle; by referring the greatest part of that pleasure, which springs out of the bosom of uneasiness, and yet retains all the features and symptoms of distress and sorrow, to the bewitching power of that eloquence, with which the melancholy scene is represented. The effect, he says, is like to the composition of two forces, which, combining together, produce a new direction, a direction not contrary to that of either, but partaking of both.

Of these four illustrations of the question, the *first* and the *last*, viz. of Du Bos and Hume, require a particular discussion, in the progress of which, the truth will probably unfold itself.

It is a misfortune, in moral as in natural philosophy, that the theory, which is to account for important phenomena, is often the creature of a bold and lively imagination, and not the modest result of careful observation and experiment. As a theory, which is built on this solid ground of observation and experiment, will always follow us downwards to the explanation of facts; so every fanciful system is found to decline this test; and, if compelled thereto, confesses its insufficiency to account for the phenomena of nature.

It is, therefore, a general objection to both these systems, in the first view of them, that the principles, to whose operation they ascribe such singular effects, are either not at all present to the mind, while their influence is supposed to be exerted and felt; or make a very dubious appearance, and utterly vanish at that critical moment, when the effect of the tragic imitation is the greatest; viz. when their presence ought to be most conspicuous and manifest to the very sense. Who in the moment, when the heart is rent and agonized with a tragic scene, can say to himself: that he converts the exquisitely painful feelings of that moment, into the character of pleasure; or so as to be attracted by, and be passionately interested in the scene; because his soul abhors the languor of indolence, and delights to be violently moved? Or who, with Mr. Hume, is ruminating on the ingenuity and eloquence of the artist, which can give to a fictitious scene all the glowing colours of nature? Who, under the possession of sympathetic sorrow, has his eye fixed upon an object of intellectual taste, and feasts in proportion to the opinion which he has of the poet's skill?

If the system of Du Bos be true, our attraction to spectacles of a tragic character, and the interest which we feel in the representation of them, ought to be proportioned to the perturbation of the mind, and to the violence of the emotions which are excited. But the most violent emotions shall be attempted to be raised, while we are only disgusted with the scene; because the whole is destitute of that single requisite, which alone has power to attach us to misery. The play of the Libertine abounds with scenes, which address themselves to our terror and indignation; but we abhor the scenes, because they exhibit no field for a benevolent compassion; they are not the tragedy of a man, but of a fiend; it is not human nature, but hell, which is exhibited. There is enough of violent action, enough of terror and distress, to rouse and agitate; but being out of the field of man, we cannot sympathise; or our horror and indignation are stronger than our sympathy, and we detest a picture, which awakens not those divine feelings, to which the soul of man delights to commit itself.

Otway, the eldest son of Shakespeare, has greatly offended in this view, and greatly lessened the impression of his genius, by the immorality and profligacy of his principal characters. We cannot feel for them as we wish, and our interest in their sufferings is diminished, as they appear to deserve them. Some touching pictures of innocent and virtuous distress, to which a pure and benevolent sympathy attaches itself, have rescued him perhaps from our utter disgust. The innocent, the gentle Monimia, and the more dignified virtue of Belvidera, relieve the horror of the villains with whom they are unhappily associated, and support in us an interest through the whole drama.

The tragedy of the Robbers, and other productions of the German drama, have the vice of Otway, but with more extravagance and scorn of nature, and therefore are more repulsive of the heart. With them to create what God never designed, and what human wickedness never meditated, is Genius; and to terrify is Sublime. If the system of Du Bos, therefore, be true, these are the perfection of the drama; but, in defiance of his principle, they do not attract, they repel; and their admirers mistake astonishment for an impression of the grand; and a horror and revulsion from scenes of dreadful suffering, for an impression of the pathetic.

But on this system, the interest in the representation ought to be proportioned, not only to the bustling of the scene, but to the bustling disposition of the spectators. This must be allowed, if the abhorrence of indolent repose, and the delight in being stirred, be the secret cause which attracts us to misery; and derives to us a pleasure in spectacles, which in their nature are painful. But this is utterly contrary to fact; and the attention to fact, in this instance, as in what I have already noticed, will demonstrate the incompetence of Du Bos' theory; and discover the true source of the phenomenon. Men of the most active turn, who with hardly any other motive than to follow the violent impulse of their own turbulent spirits, can throw society into convulsions, and feast as it were on those continually renewed scenes of distress and terror which mark their ferocious path, are not the persons on whom you would expect the representations of tragedy to produce their natural and most powerful effect; but the gentle, the flexible, the compassionate, and benevolent. The former resemble the characters which, in the introduction to this essay, I have noticed among the Romans, Goths, Spaniards, Indians, and bull-baiting Englishmen. But the man of composed and tempered manners, in whose breast compassion, mercy, and benevolence sovereignly reign, is shocked at such characters; nor could possibly encounter their rude and brutal entertainments; yet his heart is the theatre whereon tragedy acts all her glorious wonders.

The same objection bears with almost equal force against the system of Hume. It is not the man of letters, who may be supposed to be the best judge of composition and eloquence; nor yet the man of a lively imagination, to whom the effect of tragical representations is peculiarly appropriate. Though if a heart mellowed to pity be joined to these advantages, the interest in such spectacles will perhaps receive an increase from this superadded source; but tragedy exercises her utmost power on even the unlearned and untutored, if there be found a feeling and benevolent heart.

The same judgment is farther illustrated from the powerful effect on an audience of a story happily adapted to the purpose, though the composition be materially faulty. It shall awake all the passions in which tragedy rejoices, more than all the faultless productions of the Greek and French drama. Banks and Southern are poets of but a middle fame; yet the Earl of Essex and Oronooko will dissolve an audience in tears, so long as the human heart, and the inclinations which it has received from its maker shall endure. If tragedy owe her attractions to the eloquence of the poet; it is to the eloquence of nature, not of art. An untutored genius, having strong conceptions, a heart that can enter into the feelings of a fellow heart, quick in catching the most striking features of distress, judgment to select a happy tale of virtuous suffering, and simplicity to follow nature in her plain walk, will in the fabrication of tragedy reach its highest excellence. Such was Shakespeare, and such, in a less degree, were a few of his neglected cotemporaries; it was to their exquisite sensibility, to their ignorance of art and fastidious refinement, which might have diverted them from the resistless eloquence of nature, that they owe their superiority over the lettered sons of every age and nation. But, whatever be the skill of the poet, whether that of nature, or art, or of both; this skill is not critically examined into during the representation; it is felt; it no more requires the critic's acumen to capacitate us for this effect, than the philosopher's penetration into nature to feel the lightning. It is not wisdom, but the affectation of it, which in so interesting on hour, is attentive to all the finesses, delicacies, and ingenuity of the poet. The ingenuous simplicity of a plain feeling heart is better employed; it is worth a thousand such wise ones; it is the spectator and judge, whom tragedy more delights in, from whom she will receive a more abiding sentence.

Mr. Hume very justly observes, that the force of imagination, the energy of expression, and the power of numbers, are all of themselves naturally pleasing to the mind. But the connection between this position and the following conclusion, is wide as heaven and earth, when he adds: that if the object lays hold of some affection, the pleasure still rises upon us, by the conversion of this subordinate moment into that which is predominant. The predominant emotion he assumes to be the pleasure excited by the eloquence of the artist; the affection laid hold upon is the painful sensation of the spectacle. Which of these two emotions is most likely to be predominant has been just now discussed; but that a pleasurable emotion of one kind should lay hold of a painful emotion arising from a very different source, and derive augmentation to itself from this combination, is an extraordinary position; and as contrary to the laws of nature in the moral as in the material world. Whenever contraries act upon each other, diminution and not augmentation is the result. The pain continues to be pain so long as the character of the representation is preserved, and undergoes no conversion at all.—Eloquence, on whatever subject, is pleasing, but what then? If there were not some other circumstance, some powerful law of our nature which attached us to, and gave us an interest in a subject highly painful, the pleasure merely arising from the display of talents would be less pleasant, because counteracted every moment by what is painful; it must inasmuch as the pain amounts to be diminished, and if the pain greatly over-balanced the pleasure; it might be entirely obliterated.

He reasons thus himself, when he converts the proposition; observing, that if the pleasure arising from the conduct of the representation were not predominant, the effect would be destroyed, and sorrow would absorb the mind. Before the pain had no effect upon the pleasure to destroy any part of it; but as if it were of the same family, very obligingly ministered to its increase. Mr. Hume has endeavoured to avail himself of an allusion to the well-known laws of motion; but to answer his purpose he has assumed, that they resemble two forces moving in different, but not opposite directions. But their resemblance is to two forces moving in absolutely contrary directions; the effect of which is, that the greater force continues indeed to move onward in its proper direction, but with a diminution of force, equal to that of the lesser.

It is farther to be observed, on the theory of Hume, and indeed of both the French philosophers, that the one principle which satisfactorily accounts for the whole phenomenon, must be supposed to be admitted. If not, what is meant by the concession of each, that the objects represented are in their nature painful to the minds of the spectators? And why painful, but from the power of that sympathy, which enters into fellow suffering? There is no other principle in man adequate to the effect. If this be not allowed as an overpowering law of human nature, no account can be given, why a being, interested in himself, and averse to pain, should transplant into his own breast the pain of another, and court a partnership with affliction. Mr. Hume may be supposed, though perhaps involuntarily, and while nature, not theory, was speaking, to have conceded something more than this; when he observes that the pleasure, notwithstanding the supposed conversion, wears the features and outward symptoms of sorrow and distress.

Again, if to the eloquence of the poet, as to its proper cause, be ascribed the pleasure, or to speak more properly, the interest which we take in tragedy; why will not eloquence, employed on other subjects, equally interest and captivate us? Why will not pictures of other objects, equally just and animated, equally engage our affections? If it be said, that the objects must be in themselves interesting, then the effect is not derived wholly nor principally from the eloquence and manner of the artist, but from some other consideration, which previously interests us in the objects that are represented.

Mr. Hume has very artfully managed his illustration from facts, nor is it to be denied that the principles, to which he ascribes the whole, have their influence. But with less attachment to a pre-conceived theory, it is, methinks, impossible to avoid the discovery of the great master principle, even in the very facts which he himself adduces.

The notion of conversion, which he borrows from Fontenelle, is an arbitrary assumption; and may be classed with the Cartesian notion of nature's abhorrence of a vacuum. But if, by the delight of being moved be understood, that every passion of the soul delights in its proper exercise; it is true that the mind of man will, under the impulse of any affection, be moved towards the object that is united to the affection. And it is also true, that rich imagery, strong expression, the harmony of numbers, and the charms of imitation, are all grateful to the mind, independant of any end to which they are directed. It is to these that certain productions owe all their interest; but they are light auxiliaries in the grander productions of tragedy, unless so far as they are necessary to the perfection of the imitation; without which there is no representation adapted to nature, and therefore nothing fitted to lay hold of the heart.

What Mr. Hume intends by his reference to the deserted parent, is difficult to say; when he asks the question, Who would ever think of it as a good expedient for comforting an afflicted parent, to exaggerate, with all the force of oratory, the irreparable loss, which he has met with by the death of a favourite child? Certainly it were a ridiculous part to think of comforting the parent by any such means; but the idea of comforting by such a procedure, or the idea of comforting in the thing to be illustrated by this allusion, if any thing be meant to illustrate, are quite out of the question. No one ever conceived the intention of the tragic poet to be, to comfort his audience; he means to distress them; he exerts the utmost force of his genius to distress them; to give them as touching a feeling of the sorrow which he paints, as mere sympathy is capable of receiving. So is it with the deserted parent: He who would ingratiate himself with him cannot take a more effectual means than to catch him in his tenderest moments, and with all the eloquence of words, expatiate on the virtues, the shining qualities, the promising hopes of his child. Such a conduct would be unkind; it would be cruel; but it would be effectual. He would win the heart of the parent, in the very moment, and by the very act, which was rending it in pieces. It is through the gate of sympathy that he gains this access to his heart; the parent embraces the man, in whom he acknowledges a fellow heart, one who appears to feel up to the very height of that sense, which he has himself of his loss.

I have bestowed, perhaps, more attention on these *systems* than they may be thought to merit; as whatever ingenuity they may lay claim to, they have little ground of experiment in human nature to stand upon. But the examination of them has answered the principal purpose. The analysis of their defects discovers in every step the real source of the whole phenomenon; and what is of more importance, it discovers the wise provision of the great author of all for conducting the œconomy of the moral as the material world. But the simplicity of nature offends some; to discover only what every one may discover, and what nature forces upon the notice of all, argues no superiority of genius; they suppose, they invent, they create, and in defiance of nature they erect vain monuments of their own wit and ingenuity.

In every view of the human mind, during the exhibition of tragic imitations, compassion, or sympathy in a more extended sense, presents itself as the operating principle, the immediate sense to which such scenes address themselves. This is the only principle within us, which is sufficient to attach us to misery; to connect a being who is interested for himself, and is in the constant pursuit of his own proper happiness, to connect such a being with the unhappy, and as by an irresistible impulse introduce him to a partnership in their afflictions.

The contradiction, therefore, which this propensity, at the first view, carries with it to a leading principle of our own natures, vanishes when we consider it in this important light; we appear to act in perfect consistence with an acknowledged, and powerful, and highly valuable principle of our natures. While our other senses are continually opening themselves to their proper objects, it would be strange, indeed, if this internal sense, whose aim is directed to the noblest character of man, were reluctant to its proper exercise, and averse to those objects and to those scenes, which immediately address themselves to it. This would argue indeed a defect in his constitution, such as could not easily be reconciled to our ideas of that designing wisdom, which intended him to be one beautiful and harmonious whole.

If, indeed, the end of compassion, as a principle of human nature, were directed only to particular exigencies in human life, as an instant stimulus to acts of kind protection, and humane alleviation of fellow misery; it might be thought sufficient if it were reserved for such interesting occasions; and the mind were not led by a farther impulse to the participation of distress, when no immediate object of our benevolent interposition is before us. But compassion was implanted in us with more extensive views, not merely that it might come in aid of our good will on pressing occasions, which may justify the pain it gives us; but that, by a more regular and uniform exercise, it might minister to the sublimest virtue of man, and dispose us, on every occasion, to wish and do well to the creature like ourselves.

There is a striking difference in the exercise of this sense, as referred to the real distresses of human life, and to the fictitious ones of tragedy; and this difference is wisely adapted to their respective uses. When we are summoned to immediate action, the sympathetic feeling is pain unmixed, in order to give power and velocity to the benevolent stimulus. We have no propensity, therefore, to such scenes; we do not wish them to exist, in order that our compassion and benevolence may have a field to action; though he who orders, or rather permits them, has wisely provided that the calamities of human beings shall operate to the moral improvement and perfection of their minds. But where the distress is merely fictitious, or the representation of what is past; and no kind humane interposition is expected from us, but only the cherishing an uniformly benevolent temper may be supposed to be in view; then the pain is mixed and tempered with something that we know not to give a name to, something that must attend on every mind in the exercise of its best affections, a complacence such as a superior spirit may be supposed to feel, if he were viewing the infirmities and distresses of some inferior system. To such scenes, which imply no augmentation of the real calamities of our fellow creatures, but may minister to the augmentation of our good will towards them, we are moved by an internal impulse; by an impulse which we approve of in reflection, and which those who are little accustomed to reflection do however obey.

In speaking of the affection of the mind to tragic representations, I have adopted the language of the writers I have opposed, while I was discussing their theories; and I may myself, in contemplating the impulse to tragical representations, and the complacence in those benevolent affections which are excited, and distinguishing these from the impressions on the heart which the spectacle of pain excites, have used the term pleasure, yet with a visible dubiousness and reluctance, because language immediately suggested no other term; though it by no means corresponded to my idea, nor to the real truth of the sensation. When the mind is but moderately

[illegible]

interested in any tragic scene, and has leisure to attend to no other circumstances than what are appropriate to sympathy, it may be sensible to feelings which are in their nature pleasant, but chiefly, if not entirely, springing out of these collateral circumstances. But when the increasing distress of the scene entire possesses the mind, all semblance of pleasure vanishes, and the feelings are those of pure compassion; but not unless in some particular instances, painful up to the degree of aversion. It is not strictly just, therefore, to say, that the feelings at such an instant are in any degree pleasant; as it would be grossly false to say, that we are instigated to this participation of distress by the view of pleasure; unless all the sympathetic feelings be referred to the class of the agreeable ones. We are carried, indeed, by a virtuous impulse to converse with distress; the certainty that we shall not be spectators of any real suffering, withdraws all aversion to this impulse; but under this assurance we surrender ourselves up entirely to the poet; we enter into his views; we are carried out of ourselves into his fictitious scenes, as if they were real. We often feel from them an exquisite pain, which oppresses our minds for a considerable time after the representation is over, and sinks too deeply into those of a delicate and susceptible make. Yet we return to such scenes; not that pain is desirable, not to seek for pleasure in the field of pain; but the better inclination of our natures determines our conduct; and the distressing sensations, to which we are exposing ourselves, appear with that softened aspect, that grace, which a virtuous and benevolent melancholy always wears.

This investigation of the effect of tragedy on the mind, will account, in a great measure, for the superiority of the best productions of the moderns above those of the ancients, and of the English tragedy above that of the French. The pictures are more exquisitely finished; the characters of the sufferers are more interesting; and more powerfully lay hold on our affections, and plead for our compassion. Domestic life and domestic manners were more gross and undressed among the antients; the social passions were but half awakened among them; and, therefore, the pictures of domestic happiness are not near so interesting, nor can, to our improved taste, present such rich subjects of compassion. The French tragedies are in this respect also far inferior to the English; wit, gallantry, and philosophic declamation, are more displayed than touching scenes of pure and ingenuous distress. Tragedy, in order to be perfect, ought to be throughout an animated picture; enlivened, en-

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riched by grandeur of sentiment, by every exhibition of mind which is fitted to interest a fellow mind; but still it must be a picture. When this is conducted by a masterly artist; it is then that all yield to the genius of tragedy; we feel that there is an eloquence in the exhibition of virtuous distress, suffering from the incidents of our natures, from the pardonable errors of human judgment, from the follies or vices of others, or under the iron hand of oppression and cruelty, which mocks all the power of wisdom to equal; which the lettered and the polished can no more resist than the most uncultivated child of nature. And this eloquence is the instrument of a wise providence, whereby he forms and fashions our hearts according to what he designs and approves, and calls forth those benevolent affections which move not at the voice of reason and calm philosophy.

Experiments and Observations to determine whether the Quantity of RAIN *and* DEW *is equal to the Quantity of* WATER *carried off by the Rivers and raised by Evaporation; with an Enquiry into the* ORIGIN *of* SPRINGS.

BY JOHN DALTON.

READ MARCH 1, 1799.

It is scarcely possible to contemplate without admiration the beautiful system of nature by which the surface of the earth is continually supplied with water, and that unceasing circulation of a fluid so essentially necessary to the very being of the animal and vegetable kingdoms takes place. Naturalists, however, are not unanimous in their opinions whether the rain that falls is sufficient to supply the demands of springs and rivers, and to afford the earth besides such a large portion for evaporation as it is well known is raised daily. To ascertain this point is an object of importance to the science of agriculture, and to every concern in which the procuration and management of water makes a part, whether for domestic purposes or for the arts and manufactures.

For the sake of perspicuity I have distributed the subject under four heads:

1. Of the Quantity of Rain and Dew.
2. Of the Quantity of Water that flows into the Sea.
3. Of the Quantity of Water raised by Evaporation.
4. Of the Origin of Springs.

SECTION I.

An Estimate of the Quantity of Rain and Dew that falls in England and Wales in a year.

Rain-gages have been fixed of late years in almost every part of the kingdom ; by means of them we are enabled to determine, with considerable exactness, the depth of water that the rain yields in any given place. Inland counties have less rain than maritime ones, especially those which border on the western seas. But a still greater difference seems to take place between a mountainous country and a champaigne, or flat country: In the former there often falls double or triple the quantity of rain in a year, that there does in the latter, and never less than an equal quantity. It may be observed, that several years account of the rain at any place is required before a medium yearly quantity can be obtained with sufficient accuracy. The following is perhaps the largest collection of accounts of rain fallen in different places in England that has hitherto appeared: They are mostly taken from the Transactions of the Royal and other Societies.

Counties (maritime).	*Places.*	Mean annual depth in inches.
CUMBERLAND	Keswick, 7 years	67. 5
	Carlisle, 1 year	20. 2
WESTMORLAND	Kendal, 11 years	59. 8
	Fell-foot, 3 years	55. 7
	Waith Sutton, 5 years	46
LANCASHIRE	Lancaster, 10 years	45
	Liverpool, 18 years	34. 4
	Manchester, 9 years	33
	Townley	41
	Crawshawbooth, near Haslingden, 2 years	60
GLOUCESTERSHIRE	Bristol, 3 years	29. 2
SOMERSETSHIRE	Bridgewater, 3 years	29. 3
CORNWALL	Ludguan, near Mount's Bay, 5 years	41
	Another place, 1 year	29. 9
DEVONSHIRE	Plymouth, 2 years	46. 5
HAMPSHIRE	Selbourne, 9 years	37. 2
	Fyfield, 7 years	25. 9
KENT	Dover, 5 years	37. 5
ESSEX	Upminster,	19. 5
NORFOLK	Norwich, 13 years	25. 5
YORKSHIRE	Barrowby, near Leeds, 6 years	27. 5
	Garsdale, near Sedbergh, 3 y.	52. 3
NORTHUMBERLAND	Widdrington, 1 year	21. 2

Counties (inland).	*Places.*	*Means.*
MIDDLESEX	London, 7 years	23
SURREY	South Lambeth, 9 y.	22. 7

HERTFORDSHIRE	Near Ware, 5 years	25
HUNTINGDONSHIRE	Kimbolton, 7 years	25
DERBYSHIRE	Chatsworth, 15 years	27. 8
RUTLANDSHIRE	Lyndon, 21 years	24. 3
NORTHAMPTONSHIRE	Near Oundle, 14 years	23
	General Mean	35 2

This general mean of 35. 2 inches is, I apprehend, a little above the medium for England and Wales, as the greater number of places are those where much rain falls. If we take a mean for each of the above-mentioned counties (where more than one place in a county is given) and then a general mean from the counties, the result is a reduced mean of 31. 3. Even then it may be objected that the greater part of the counties are maritime; but it must be observed, that there is no account of rain in Wales; and we may safely conclude, that the rain in Wales would exceed the last-mentioned mean as much as the inland counties of England, not in the above list, would fall short; because Wales is both a mountainous country, and exposed to the sea.

We will, therefore, conclude, that the mean annual depth of rain in England and Wales, deduced from these 20 counties, is 31 inches: A quantity which subsequent observations, I am confident, will not diminish, and probably not increase much.*

It remains to estimate the quantity of dew that falls in a year.—Some have doubted whether dew is derived from the air or the earth; but a proper attention to the phenomena will satisfy us, that it is a deposition of water, evaporated during the heat of the day. With respect to the quantity that falls in a year, we are much at a loss, as no daily observations have been made for a series of time that I know of: indeed, it would be difficult to prescribe a mode of observation. Dr. Hales† relates some experiments made to determine the quantity of dew that falls upon moist earth, from which he estimates the annual dew at 3. 28 inches. But it is probable that the dew which is deposited on grass is much more copious than what falls on moist earth, because grass exposes much more surface in a given acre of ground. If we take the dew at 5 inches annually, it will probably not be much over-rated: supposing it should be over-rated, the excess may stand against the rain that is lost by evaporation from the surface of the rain-gage each time it rains.* Wherefore, upon the whole, we

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* The editors of the Encyclopedia, under the article Weather, from 16 places of observation, make the annual mean for Great Britain 32. 53 inches; and M. Cotte, in the Journal de Physique for 1791, gives a mean derived from 147 places in different parts of the world equal to 34. 7 inches.

† Veg. Statics. Vol. 1. Page 52.

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* Since writing the above paragraph on dew, I have had occasion to make several experiments on the subject of aqueous vapour, as it exists in the atmosphere, the result of which will, I am persuaded, materially illustrate this important question in physics.—At present I shall only observe, that the following conclusions seem deducible from the experiments above referred to.

1. That aqueous vapour is an elastic fluid *sui generis*, diffusible in the atmosphere, but forming no chemical combination with it.

2. That temperature alone limits the *maximum* of vapour in the atmosphere.

3. That there exists at all times, and in all places, a quantity of aqueous vapour in the atmosphere, variable according to circumstances.

4. That whatever quantity of aqueous vapour may exist in the atmosphere at any time, a certain temperature may be found, below which a portion of that vapour would unavoidably fall or be deposited in the form of rain or dew, but above which no such diminution could take place, chemical agency apart. This point may be called the *extreme temperature* of vapour of that density.

5. And that whenever any body colder than the extreme temperature of the existing vapour is situated in the atmosphere, dew is deposited upon it, the quantity of which varies as the surface of the body and the degree of cold below the extreme temperature.

N.B. The *extreme temperature* of vapour in the atmosphere varies all the way from the *actual* temperature of the atmosphere to 10, 15, 20 or more degrees below it.—The point may generally be found in the hottest months shall have 36 inches of water at a medium annually on the surface of the earth in England and Wales, reckoning 31 for rain and 5 for dew.

According to Guthrie, the area of England and Wales is 46.450 square miles. This reduced to square feet, gives 1.378.586.880.000: which, multiplied by 3 feet the annual depth of rain and dew, gives 4.135.760.690.000 cubic feet of water=153.176.320.000 cubic yards, or 28 cubic miles=115 thousand millions of tons in weight, nearly.—We must now consider how this enormous quantity of water is disposed of.

There are two principal ways by which the water derived from rain is carried off again: One part of it runs off immediately into rivulets, or sinks into the earth a small way, breaks out again in lower ground in the form of springs, thence makes its way to some river, by which it is conveyed into the sea—another part is raised into the atmosphere by evaporation. We take no notice here of the decomposition of water by vegetables; because it is presumed that in the course of nature the principles are combined and water formed again.

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by pouring cold spring water into a dry and clean glass, and marking what degree of cold is sufficient to produce a dew on the outside of the glass; at other times frigorific saline solutions may be used.

§ 2.

An Estimate of the Quantity of Water that flows into the Sea from England and Wales in a Year.

To calculate the quantity of water that flows down any one river into the sea in a given time, seems at first view a question of great difficulty. The necessary data, however, may be obtained with considerable exactness, by proper observations, and then it becomes an easy case of mensuration. Dr. Hutton, in his Philos. and Mathemat. Dictionary, article River, proposes a very good method to determine by experiment the velocity of a river:—A cylindrical piece of light wood, its length somewhat less than the depth of the waters, is to be taken, and a few small weights attached to one end in order to make it swim upright. To the other end a small rod is fixed in the centre in direction of the axis.—This being suffered to float down the stream will move with the velocity of the water; and if the rod be observed to incline towards the river upward or downward, it shews the current to be more rapid at the bottom or surface respectively.

This experiment being made in the middle and near the sides of a river, a medium velocity may be obtained. Then the medium, breadth, depth, and space run over in a certain time being multiplied together, will give the quantity of water that flows down in that time.

Dr. Halley, in order to estimate the quantity of water that flows into the Mediterranean Sea by means of rivers, makes a comparison of the great rivers of Italy, &c. with that of the Thames. (Philos. Transact. Abridg. Vol. 2. Page 110). He assumes the breadth of the Thames at Kingston Bridge to be 100 yards, its depth 3 yards, and velocity 2 miles per hour. He professedly overrates the dimensions, in order to allow more than a sufficiency for the streams received below Kingston. This assumption gives the area of a transverse section of the river=300 square yards, and the quantity of water flowing down=20.300 000 tons in a day. This must be overrated by at least, I think, one third:—If the breadth be assumed 100 yards, the depth 3, and velocity 2 miles per hour, it will then give $\frac{2}{3}$ of the result above mentioned; or it will amount to the same thing if we take $\frac{1}{8}$ part from all the three data assumed by Dr. Halley, the result being $\frac{2}{3}$ of that above; amounting in the year to 166.624.128.000 cubic feet, which is a little more than $\frac{1}{25}$ part of all the rain and dew in England and Wales in a year, as above deduced.

By an inspection of the annexed map of the rivers of this country, as well as by a fair calculation, it appears, that the water of the Thames is drawn from an extent of country of about 600 square miles, or $\frac{1}{8}$ of the area of the whole, nearly. The Severn, including the Wye, spreads over an equal or greater extent of country: And that collection of rivers which constitutes the Humber is superior to either of the other two in this respect. As far as my own observation goes, the Severn and Wye must disembogue as much or more water than the Thames; the Humber I have not seen collectedly, but have noticed most of the branches constituting it, and should apprehend it can not be inferior to the Thames: All other circumstances being the same, the quantity of water carried down by any river should be as the area of the ground from which the water is derived, and on this account the Humber ought to exceed the Thames.*

The Severn, which is partly derived from the mountainous country of Wales, is certainly the most rapid of the three rivers, and probably carries down the most water: As the Thames, however, is generally considered to take the lead, we will suppose, upon the whole, that these three rivers are equal in this respect.

The counties of Kent, Sussex, Hampshire,

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* A more perfect theorem will be given afterwards, for finding the quantity of water carried down by any river.

Dorsetshire, Devonshire, Cornwall, and Somersetshire, from the Medway to the lower Avon inclusively, in an extent of 11.000 square miles, do not present us with many large rivers. From their number and magnitude, we cannot form a high estimate of their produce. The quantity of rain for those counties is indeed near the average for the kingdom, as far as the preceding observations determine; but the milder temperature of their winters and greater heat of their springs and summers, will cause a greater evaporation than in some other parts: It is probable the rivers in these counties may amount, when taken together, to 1½ times the magnitude of the Thames. The rivers that disembogue their waters on the coast of Lincolnshire, Norfolk, Suffolk and Essex, from the Humber to the Thames, though drawn from a country of 7000 square miles, manifestly fall far short of the Thames. The two places in this district, for which we have accounts of the rain, Norwich and Upminster, give a mean of only 22½ inches annually. This, with the flatness of the country, which prevents the water from running off in some degree, makes the rivers much less than what might otherwise be expected from the extent of ground. There are but 3 or 4 of any consequence. Probably all the rivers may amount to half the size of the Thames. There remains above 6000 square miles in Wales, from the Wye to the Dee, inclusive of the last, and the northern counties of Lancaster, Westmorland, Cumberland, Northumberland, and Durham, with part of Cheshire and a small part of Yorkshire, from the Mersey round by the Tweed to the Tees, amounting to 7 or 8000 square miles, to be estimated.

These two divisions, though not larger than some others, abound in rivers, many of which are considerable in magnitude and of great rapidity. The rains at an average, it is probable, are double what they are in the S. E. counties of the kingdom. The rivers in these two districts cannot fairly be estimated, I think, at less than *four* times the Thames.—It appears, then, that by this estimation, the water carried off by all the rivers in England and Wales, may amount to *nine* times that carried off by the Thames = 13 inches of rain. There remains still *sixteen* times the water of the Thames, or 23 inches of rain to account for, before we have disposed of all the rain and dew.

§ 3.

An Estimate of the Quantity of Water raised by Evaporation.

Upon looking over the surface of any country, three principal varieties of surface present themselves to view, as far as respects evaporation, namely, *water*, ground covered with grass and other vegetables, and bare soil. The difficulties that occur in attempts to find the quantity of water evaporated in those three cases, are perhaps the principal reason why our knowledge on this head is so imperfect.

As far as experiments hitherto made authorise us to draw conclusions, it should seem that the evaporation from water is greatest; that from green ground is probably next, and that from bare soil the least: though we may presume, that the copious dews upon the grass more than supply the excess of evaporation above what takes place from a moist uncovered soil.

The most satisfactory experiments I have seen an account of, relating to the evaporation from a surface of water, are those of Dr. Dobson, made at Liverpool, in the years 1772, 73, 74 and 75. (Vid. Philos. Transac. Vol. 67) —He took a cylindrical vessel of 12 inches diameter, and having nearly filled it with water, exposed it besides his rain-gage of the same aperture, and by adding water to it, or taking it away occasionally, he kept the surface nearly of the same height, and carefully registered the quantities added or taken away, by a comparison of which with the rain, the amount of the evaporation was ascertained. The mean monthly evaporation for 4 years was—January 1.50 inches.—February 1.77.—March 2.64.—April 3.30.—May 4.34.—June 4.41.—July 5.11.—August 5.01.—September 3.18.—October 2.51.—November 1.51.—December 1.49.—In all 36.78 inches. The mean rain for the same time was 37.48 inches.—In the year 1793 I found the evaporation from water in a similar way at Kendal for 82 days in March, April, May and June to be 5.414 inches. The greatest quantity evaporated on one of the hottest and driest days in Summer was a little above ,2 of an inch in depth.

The experiments to determine how much is evaporated from green ground and from moist earth, are very few that have come to my knowledge. Dr. Hales, from a few experiments, calculates that moist earth only throws off $6\frac{2}{3}$ inches annually.—This calculation must be far below the truth. Dr. Watson, Bishop of Llandaff, found that in a dry season there evaporated from a grass plat that had been mowed close, about 1600 gallons in an acre per day, which amounts nearly to ,07 of an inch in depth; and that after rain the evaporation was considerably more. Now supposing ,07 to be the medium daily evaporation for May, June, July and August, and that as much is raised in these 4 months as in all the rest of the year, the annual evaporation in such circumstances will be 17 or 18 inches, which is but half that observed from water at Liverpool, and 6 inches less than the reserve of rain stated above.

In order to ascertain this point more fully, and to investigate the origin of springs, my friend Thomas Hoyle, jun. and self practised an expedient as follows, beginning in the autumn of 1795. Having got a cylindrical vessel of tinned iron, 10 inches in diameter and 3 feet deep, there were inserted into it two pipes turned downwards for the water to run off into bottles: The one pipe was near the bottom of the vessel; the other was an inch from the top. The vessel was filled up for a few inches with gravel and sand, and all the rest with good fresh soil. It was then put into a hole in the ground and the space around filled up with earth, except on one side, for the convenience of putting bottles to the two pipes; then some water was poured on to sadden the earth, and as much of it as would was suffered to run through without notice, by which the earth might be considered as saturated with water. For some weeks the soil was kept above the level of the upper pipe, but latterly it was constantly a little below it, which precluded any water running off through it. Moreover, for the first year the soil at top was bare; but for the two last years it was covered with grass the same as any green field. Things being thus circumstanced, a regular register has been kept of the quantity of rain water that ran off from the surface of the earth through the upper pipe (whilst that took place) and also of the quantity of that which sunk down through the 3 feet of earth, and ran out through the lower pipe. A rain-gage of the same diameter was kept close by to find the quantity of rain for any corresponding time.

The following Tables shew the Result.

	Water through the two Pipes.			Mean.	Mean Rain.	Mean Evap.
	Inch.	Inch.	Inch.	inch.	inch.	inch.
	1796.	1797.	1798.			
Jan.	1.897—	,680—	1.774+	1.450+	2.458	1.008
Feb.	1.778—	,918—	1.122	1.273	1.801	,528
March	,431—	,070—	335	.279	,902	,623
April	,220—	,295—	,180	,232	1.717	1.485
May	2.027—	2,443+	,010	1.493+	4.177	2.684
June	,171—	,726	——	,299	2.483	2.184
July	,153—	,025	——	,059	4.154	4.095
Aug.	——	——	,504	,168	3.554	3.386
Sept.	——	,976	——	,325	3.279	2.954
Oct.	——	,680	——	,227	2.899	2.672
Nov.	——	1.044	1.594	,879	2.934	2.055
Dec.	,200	3.077	1.878+	1.718+	3.202	1.484
	6.877—	10.934—	7,379	8.402	33.560	25.158
Rain	30.629—	38.791—	31,259			
Evap.	23.725—	27.857—	23.862			

The following observations were made when the water passed through both pipes: that is, when the vessel was filled up with earth above the level of the upper pipe.

		Top pipe Inch.	Bottom pipe. Inch.
1796. Jan.	25	,190	,280
	30	,080	,114
Feb.	2	,100	,254
	8	,196	,140
May	1	,163	,000
	10	,060	,400
	12	,312	,175
	15	,190	,200
June	3	,120	,040
	Total	1.411	1.603

The column of mean evaporation is derived by taking the difference of the two columns preceding it; but it should be observed that though this method is sufficiently exact in taking the year together, it is not so in taking the months severally, because it presumes that the earth in the vessel contains the same quantity of water at the end of each month, or is saturated with it; whereas in the Summer months it is frequently short of saturation. The consequence is, that the evaporation appears from this table to be something less than it really is in the Summer months, and something more in the autumnal.*

From these experiments it seems we may conclude—1st, That the quantity of water evaporated, *in the circumstances above related*, amounts to 25 inches of rain annually; to which if we add 5 inches for the dew, it will give 30 inches of water raised annually.

2d. That the quantity of evaporation increases with the rain, but not proportionally. Thus, 1797 gave the most rain and the greatest evaporation, &c.

3d. That it does not appear there is much difference betwixt the evaporation from bare earth, when there is sufficient depth of soil, and that from ground covered with vegetating grass. The account in 1796 is much what might have been expected, if the earth had been covered with grass.

As this account of evaporation, amounting to 30 inches, exceeds the medium reserve of rain of 23 inches, it demands an enquiry whether the rain is adequate, or whether the earth derives a supply of water from some subterranean reservoir, according to the opinion of some philosophers.

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* N. B. The earth in the vessel always appeared as well supplied with moisture as the ground around it, in the driest weather.

With respect to the deficiency of 7 inches, there are *three* causes to be assigned for it, which appear to me fully sufficient, without having recourse to any source but that of rain for the supply of the earth in general.

1st. In the account of the rain that passed through the earth in our evaporating vessel, there are a few monthly products marked,+ those were occasioned by the bottle that received the water through the pipe being found with the water running over; this loss was placed to the account of evaporation; it could not be much, as the water was taken several times in a month, but possibly might amount to one inch in the year.

2d. The rain at Manchester, being 33½ inches annually, exceeds the medium of 31 inches; and consequently, according to the preceding observations, the evaporation ought to exceed the medium.

3d. But the principal cause of the excess in our account of evaporation, I conceive to be the prevention of the water running off from the surface of the earth at the top, by having the earth below the level of the upper pipe: It has been seen, that when the earth was above that level, a great part of the water came off that way, by which the surface was sooner dried: whereas by forcing all the water to sink through the earth or stand on its surface, a greater degree of moisture perpetually existed at the surface, and consequently afforded a greater scope for evaporation, than the surface of the earth in general would do.

Upon the whole then I think we may fairly conclude—that the *rain* and *dew* of this country are equivalent to the quantity of water carried off by evaporation and by the rivers. And as nature acts upon general laws, we ought to infer, that it must be the case in every other country, till the contrary is proved.

This conclusion being admitted, we are enabled to deduce a general theorem for the quantity of water carried down into the sea by any river in any country (on the supposition that all rivers are ramified alike) provided we have certain *data*: these *data* are the length of the river, and the excess of the rain above the evaporation in the country from which the water of the river is drawn: Also, it should be known by observation, how much water some one given river carries down.

For, from the principles of geometry, the area of country from which any river is supplied, will be as the square of the length of the river; and the quantity of water carried off, will be in the compound ratio of the area of the country, and the excess of the rain and dew above the evaporation.

Thus, let L=the length of any river, E=the excess of rain and dew above the evaporation, and Q=the quantity of water disembogued in any given time by that river; l=the length of any other river, e=the excess, &c. and q=the quantity of water; then we shall have q=

$$\frac{Q\,l\,e}{L\,E}$$

Ex. gr. Suppose the length of the Thames =200 miles, and the excess=5 inches, estimating the rain and dew at 30 inches and evaporation at 25; and suppose the river Kent, in Westmorland, to be 20 miles in length, and the excess 35 inches, the rain and dew being supposed 65, and evaporation 30 inches.

$$\text{Then, } \frac{20^2\times35\times Q}{200^2\times5}=\frac{7\,Q}{100}=q$$

or Q=$14\frac{2}{7}$q; which result, I believe, will be found to accord nearly with the measurement of the two rivers on the principle before mentioned.

§ 4.

On the Origin of Springs.

The Origin of Springs has always been justly considered as a question of natural history worthy of investigation.—In the infancy of science hypotheses are formed to account for phenomena; but when facts are discovered totally inconsistent with an hypothesis, it ought to be discarded. This does not seem to have been the case in the subject before us; for various opinions are still held by some, which it is impossible to support by facts. The object of the following remarks and experiments is to ascertain the disputed point if possible.

There are *three* opinions respecting the origin of springs which it may be proper to notice.

1st. That they are supplied entirely by rain and dew.

2d. That they are principally supplied by large subterranean reservoirs of water.

3d. That they derive their water originally from the sea, on the principle of filtration.

It is obvious, that before we pay any attention to the two latter opinions, the causes assigned in the first ought to be proved insufficient by direct experiment. M. de la Hire is the only one who has attempted to do this, as far as my information extends, in the Parisian Memoirs for 1703. He procured a leaden vessel 8 feet deep, having a pipe at the bottom; this he buried in the earth, and filled with soil of sand and loam, exposing the surface to receive all the rain that fell. After 15 years trial, he found that no water had run through the pipe at the bottom.

Again, he took another vessel, 8 inches deep, which he filled with earth and exposed in like manner. No rain penetrated so as to run out at the bottom from June to February; but after that time it yielded a quantity after most rains. Another vessel of twice the depth, or 16 inches, gave a result much like that of 8 inches. Farther, M. de la Hire found, that when herbs were planted in the soil of the last mentioned vessel, and grown up, no rain penetrated through the soil, but instead thereof it was not sufficient to sustain the vegetation; for the plants would require to be sprinkled occasionally, or else they began to droop and wither.

With respect to the first mentioned fact, we need not wonder that no water penetrated through 8 feet of earth at Paris, where the annual rain is but 20 inches, when only 8 or 9 inches penetrated through 3 feet of earth here, where the rain is 33 or 34 inches annually. But it does not follow that rain may not descend down declivities of the ground into vallies or lower parts, at Paris as well as here, and being accumulated may penetrate into the earth to a considerable depth, especially if it meet with channels or chasms of any kind, or declining strata of earth that are impenetrable by water. Paris, I believe, however, is not very liberally supplied with springs, as might be expected. As to the experiment upon vegetation, it only proves that the rain in spring and summer is sometimes not sufficient to support vegetable life, a fact which may readily be granted; but then in his experiment the plants were precluded from a supply of moisture from the earth beneath the vessel, which is a reserve of the utmost consequence in dry seasons.

This circumstance of water ascending again in the earth, on whatever principle it is effected, cannot be denied.—There were 4¾ inches of rain here in July last, none of which passed through the earth in the evaporating vessel; this earth, however, at the end of the month, was far from that degree of dryness which is unfit for the support of vegetation.—During the first four days of August there fell about 3 inches of rain, and only ½ an inch penetrated through the earth in the evaporating vessel. Consequently 3 feet in depth of earth that was moderately moist imbibed nearly 3 inches of rain before it was saturated; whence we may conclude that 3 inches nearly had ascended and been evaporated. This evidently shews, that earth is capable of holding a very great proportion of water, that in summer the water ascends to supply the exigences at the surface, and that earth far under the point of saturation with moisture is still fit to support vegetation.

This observation suggested the following question—How much water is there in a given depth of earth when the soil is at the point of saturation, or in that state when it begins to yield water from the lower pipe of the evaporating gage?

To determine this I took a quantity of garden soil that had been soaked with rain a day before, and pressed it into a crucible; in this state I found its specific gravity to that of water as 5 to 3. It was then exposed to a moderate heat till it appeared, as near as I could judge, of the same moisture as garden soil two inches deep in dry summer weather; afterwards it was exposed almost to a red heat till it became a perfectly dry powder; in the former case it lost $\frac{1}{12}$ of its weight, and in the latter $\frac{1}{3}$.—When it had lost $\frac{1}{6}$, it did not appear too dry to support vegetation. When it had lost $\frac{2}{9}$, it appeared like the top soil in summer.—Hence it follows, that every foot of earth in depth, so saturated, contains 7 inches of water, and that it may part with one quarter of its water, or even one half, and not be too dry for supporting vegetation.

Clay, just dug out for the purpose of making bricks, was tried in the same manner: It gave the same specific gravity as the earth, and yielded not much less water.

These experiments and observations prove, that M. de la Hire's conclusions, drawn from the vegetation of plants in a given quantity of soil, precluded from any communication with the earth at large, are erroneous, or at least unwarranted: As it does not thence appear that the evaporation for the *whole* year exceeds the rain in the year, whatever it may do for a month or two in summer.

The origin of springs may still therefore be attributed to rain, till some more decisive experiments appear to the contrary; and it becomes unnecessary to controvert the other two opinions respecting this subject.

Upon the whole it should seem, that at the commencement of spring, the ground is nearly saturated with water for 5 or 6 feet in depth, as the rains and dews in autumn and winter far exceed the evaporation: There are then 5 or 6 inches of water at least to be raised up again to the surface in case of exigence in the spring and summer: If this happen to be so, then it is at the expence of springs; for we find the generality of springs become languid, or entirely cease to flow at the end of a long drought. As to the few springs that seem to be little affected by dry or wet seasons, they form exceptions which it would not be difficult to account for.

EXPERIMENTS *and* OBSERVATIONS *on the Power of Fluids to conduct* HEAT; *with Reference to Count Rumford's Seventh Essay on the same Subject.*

BY JOHN DALTON.

READ APRIL 12TH, 1799.

THE nature and properties of fire or heat are subjects which present themselves to our consideration in almost every department of physics: It is no wonder therefore that new experiments, which point out and define the modes of operation of fire, before unobserved, or at least too much overlooked, should attract the attention of philosophers.—These observations were suggested upon reading Count Rumford's very ingenious experiments, in his essay abovementioned, which exhibit a fact in a more striking point of view than it has appeared before—namely, *that the quickness of the circulation and diffusion of heat in fluids, is occasioned principally by the internal motion arising from a change of specific gravity effected by the heat.*—But the conclusion he has drawn from them—*that fluids are perfect non-*

conductors of heat, in the way in which solids conduct it, appears to me totally unwarranted from the experiments, and erroneous in itself. And as it may be an error of practical consequence, if adopted, the exposition of it seemed desirable—which is the object of the following remarks and experiments.

My first attempt was to ascertain the precise degree of cold at which water ceases to be further condensed—and likewise how much it expands in cooling below that degree to the temperature of freezing, or 32°. For this purpose I took a thermometer tube, such as would have given a scale of 10 inches with mercury from 32° to 212°, and filled it with pure water. I then graduated it by an accurate mercurial thermometer, putting them together into a bason filled with water of various degrees of heat, and stirring it occasionally: As it is well known, that water does not expand in proportion to its heat, it does not therefore afford a thermometric scale of equal parts, like quicksilver.

From repeated trials agreeing in the result, I find, that the water thermometer is at the lowest point of the scale it is capable of, that is, water is of the greatest density at 42°½ of the mercurial thermometer. From 41° to 44° inclusively the variation is so small as to be just perceptible on the scale; but above or below those degrees, the expansion has an increasing ratio, and at 32° it amounts to $\frac{1}{8}$th of an inch, or about $\frac{1}{160}$th part of the whole expansion from 42°$\frac{1}{2}$ to 212° or boiling heat.—During the investigation of this subject, my attention was arrested by the circumstance, that the expansion of water was the same for any number of degrees from the point of greatest condensation, no matter whether above or below it: thus, I found that 32 , which are 10°$\frac{1}{2}$ below the point of greatest density, agreed exactly with 53°, which are 10°$\frac{1}{2}$ above the said point; and so did all the intermediate degrees on both sides. Consequently when the water thermometer stood at 53°, it was impossible to say, without a knowledge of other circumstances, whether its temperature was really 53°, or 32°. Recollecting some experiments of Dr. Blagden in the Philosophical Transactions, from which it appears that water was cooled down to 21° or 22° without freezing, I was curious to see how far this law of expansion would continue below the freezing point, previously to the congelation of the water, and therefore ventured to put the water thermometer into a mixture of snow and salt, about 25° below the freezing point, expecting the bulb to be burst when the sudden congelation took place. After taking it out of a mixture of snow and water, where it stood at 32° (that is 53° per scale) I immersed it into the cold mixture, when it rose, at first slowly, but increasing in velocity, it passed 60°, 70°, and was going up towards 80°, when I took it out to see if there was any ice in the bulb, but it remained perfectly transparent: I immersed it again and raised it to 75° per scale, when in an instant it darted up to 128°, and that moment taking it out, the bulb appeared white and opake, the water within being frozen: Fortunately it was not burst; and the liquid which was raised thus to the top of the scale was not thrown out, though the tube was unsealed. Upon applying the hand, the ice was melted and the liquid resumed its station. This experiment was repeated and varied, at the expence of several thermometer bulbs, and it appeared that water may be cooled down in such circumstances, not only to 21°, but to 5° or 6°, without freezing, and that the law of expansion abovementioned obtains in every part of the scale from 42°½ to 10° or below; so that the density of water at 10° is equal to the density at 75°. But as the discovery of this curious, and I believe hitherto unnoticed property, has little to do with the object before us, I shall say no more of it at present.

Count Rumford's principal experiments are those in which a cake of ice was confined on the bottom of a cylindrical glass jar, of 4.7 inches in diameter, and 14 high, and water poured upon it of different temperatures suffering it to stand, without agitation. He found that about 6lb. of boiling hot water melted little more ice than as much water of 41°; and that by making such allowances as the experiments seemed to warrant for deductions when hot water was used, water of 41°, or 9° above the freezing point, melted quite as much, and often more, than the hot water: From which he infers, that water, and by analogy all other fluids, do not transmit heat in the manner that solids do, but circulate it solely by the internal motion of their particles.

The existence of this internal motion he has proved decidedly; that water of a certain temperature being of the greatest density, will always take the *lowest* place, and water either warmer or colder than that degree will ascend. This degree of greatest condensation he takes on the authority of others at 40°; it appears however from the experiment related above, to be still more favourable to his position, namely 42°½: And that water of 32° must ascend till it comes to water of 53°, if it be not cooled in its progress, which circumstance he admits.

Upon considering the facts related in his experiments therefore, there are *three* causes which suggest themselves as conspiring to circulate and diffuse the heat, by which the ice is melted.

1st. The internal motion of the liquid, by which water of 32°, incumbent upon the ice, is perpetually ascending into a warmer region of 53°, and warmer water of 42°½ descending to take its place.

2d. The proper conducting power of the liquid independent of internal motion.

3d. The conducting power of the glass jar. But as glass is known to be a very bad conductor of heat, it can produce no material effect in these experiments: For which reason Count Rumford does not appreciate the third cause.

With respect to the operation of the first cause, it will *generally* be supposed that *cold* water rising into *warmer* and remaining with it, the heat is impaired, and the two reduced to a common temperature. But Count Rumford does not admit of this communication; he maintains, that the two still retain there proper share of heat, notwithstanding they are mixed together. This hypothesis of his is of no peculiar consequence as far as respects the effect of the internal motion: For the temperature indicated by a thermometer immersed in an equal mixture of water at 32° and 53°, would be the same as if the water was uniformly of the temperature 42°½. But it has material consequences in other respects; for, if it be admitted, it annihilates the *second* cause abovementioned, and it would follow that warm water being put upon cold water above the temperature of 42°½, the heat could not in any degree be propagated downwards, unless by agitation, and even then, upon subsiding, the warm part ought to rise to the top, and the cold fall to the bottom.

These positions are so manifestly contradictory to common opinion, that they can not be received without proof. But Count Rumford has not given us a single experiment to prove them. It seemed necessary therefore, to clear up this point by direct experiments.

Experiment 1.

Took a large tumbler glass, 3½ inches diameter, and 5 inches deep, and filled it half way with water of 51°, then gently filled up the rest by means of a small syphon, with water of 88°; a thermometer, with its bulb and stem detached from the frame, being previously immersed to the bottom. The temperatures at the top and middle were had by gently immersing the bulb of another thermometer into the water.

Time	TEMPERATURE		
elapsed.	*at top.*	*in the middle.*	*at bottom.*
——	88°	——	51°
5 min.	85	——	54
12	83	75°	56
18	80	72+	58
30	76	——	60
40	73	——	61
50	70	——	61
60	69	——	61

(Air in the room 50°.)

Experiment 2.

The same as before; only a circular piece of wood floated upon the surface of the water, on the centre of which the stream of the syphon was directed to prevent the current downwards.

(Air in the room 55°.)

	TEMPERATURE	
Time.	*at top.*	*at bottom.*
Before the water was poured on		56°
—	116°	56+
10 min.	105	56½
20	92	57 —
30	85	57⅓
40	80	57¾
50	77	58+
1 h. —	75	58½

	TEMPERATURE	
Time.	*at top.*	*at bottom.*
1 h. 10 min.	72°½	58°½
— 20	70	59 —
— 30	67	58½
— 40	65	58+
— 50	63	58
2 —	62	58 —
— 15	61	57½
— 30	59½	57
3 —	57½	56 (Air 52°)
5 —	53½	53 Do.

A similar result was obtained in a different way by the following.

Experiment 3.

Took an ale glass of a conical figure, 2½ inches in diameter and 3 inches deep; filled it with water that had been standing in the room, and consequently of the temperature of the air nearly—Put the bulb of a thermometer to the bottom of the glass, the scale being out of the water: Then, having marked the temperature, I put the red hot tip of a poker, half an inch deep into the water, holding it there steadily about half a minute; and as soon as it was withdrawn, I dipt the bulb of a sensible thermometer into the water about $\frac{1}{4}$ inch, when it rose in a few seconds to 180°.

	TEMPERATURE		
Time.	*at top.*	*middle.*	*bottom.*
Before the poker was immersed			47°
—	180°		47°
5 min.	100	60°	47½
20	70	60	49
1 h. —	55	—	52

These experiments all evidently agree in proving water to have a proper conducting power, independent of any internal motion.— It surely will not be said that any slight motion unavoidably made at the beginning of an experiment, could continue with a powerful effect for upwards of an hour.—However, to determine this matter, I made the two following experiments.

Experiment 4.

Took the glass tumbler of the first experiment, and filled it half way with rain water, deeply tinged with archil; then filled it up with clear warm water, as related in the 2d experiment.— The upper half was but just perceptibly tinged by the process and uniformly so; it remained for an hour not visibly altered in this respect, though by frequently putting the bulb of a thermometer down to the middle, the colour at last rose in a small degree.

(Air 45°)

TEMPERATURE

	at top.	*middle.*	*bottom.*
Before the warm water was poured on ..			44°
Time	105°	—	—
7 min.	97	—	47+
17	86	—	48
27	79	—	49+
37	75	68	50
47	70	66	50+
57	66	62	51½
1 h. 7	60	62	51½
— 17	60+	59	51½
— 27	59	—	51½

Experiment 5.

A glass tube near an inch in diameter, and 16 inches long, was half filled with a coloured solution of common salt in water, warm; a small thermometer was wholly immersed in it, and cold clear water carefully poured upon the whole so as to fill the tube; the colour ascended very little, and continued invariable after the process of filling.—The warm solution was of course made of greater specific gravity than the cold water.

(Air 45°)

	TEMPERATURE	
	at top.	*bottom.*
Time.	45°	85°
5 min.	53	79
10	53	74
21	52	69
31	51	66
45	50½	64
58	50	61
1 h. 31	49	56¼
3 30	47	51
— 55	47	50
4 15	—	49
7 5	46	48

To determine whether hot and cold water being suddenly mixed, and agitated, the hot would afterwards rise to the top, was the object of

Experiment 6.

Air in the room 50°.—About ½ pint of water of 130° was poured into a cold tumbler glass, and immediately after as much water of 50°; the mixture was agitated for half a minute by a deal rod; after which an immersed thermometer stood at 85°, both at top and bottom; it was then set by in a still place for examination.

	TEMPERATURE	
Time.	*at top.*	*bottom.*
15 min.	$77^{\circ}\frac{1}{4}$	77°
30	73	$72\frac{2}{3}$
45	68	$67\frac{2}{3}$
1 h. —	64.8	64.6

From all these experiments it is evident, that water has a proper conducting power: In the last experiment, if the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have risen to the top, and the cold ones subsided so as to have made a material difference in the temperature.—It is, however, equally evident, that water is a *bad conductor* of heat, probably as it is of electricity; the descent of the heat in the second experiment is wonderfully slow; a slight agitation for one *second* would do as much to induce the equilibrium as standing still one *hour*. In repeating the third experiment, in a wine glass, I have several times known water $\frac{1}{2}$ an inch deeper to differ 50° in temperature from the incumbent water.

We must conclude, therefore, that the quick circulation of heat in water over a fire, &c. is owing *principally* to the internal motion excited by an alteration of specific gravity; but not *solely* to that cause as Count Rumford has inferred.

If it be proved that water conducts heat, it will scarcely be necessary to prove, that other fluids conduct it, and that they communicate it one to another:—The two following experiments shew that mercury conducts it, and that water and mercury reciprocally communicate it.

Experiment 7.

Took a cylindrical glass tube, of 1 inch internal diameter, and put 1¾ inches in depth of mercury into it, and immersed the bulb and stem of a thermometer to the bottom, the scale as usual being above the liquid; then put 2¼ inches of warm water upon it by a syphon, and let it stand without agitation.

	TEMPERATURE.			TEMPERATURE.	
	Merc.	*Water.*	*Time.*	*Merc.*	*Water.*
Time.	56°	122	14 m.	74°½	92°
3 m.	70	118	19	73	87
6	73	110	27	71	78
11	75	100			

Experiment 8.

Into a tumbler glass, 2½ inches in diameter, poured an inch in depth of mercury, and heated it to 110°; upon which was poured an inch of water at 50°, and then kept still.

	TEMPERATURE.	
	Merc.	*Water.*
Time.	110°	50°
4 min.	74	70
8	71	70½
10	70½	70

Finding that water was so bad a conductor of heat, I was desirous to learn how ice would conduct it, and tried it as follows.

Experiment 9.

Feb. 9th. Out of a mass of ice, by means of a hot iron, I shaped a cylindrical piece, 3 inches in diameter, and 5½ inches long, clean and pure; its weight 17 ounces. Made a small round hole at one end, one inch deep, and the size of a thermometer bulb, which was inclosed in it.—The other end of the piece was put into a bason of snow and salt, to the depth of from ½ to 1½ inches, the temperature of which was kept below 10° for 1¾ hours. Air 37°.

Time elapsed.	Therm. in the liquid.	Therm in the ice.
	5°	32°
1¾ h. at a medium	7°	31½

N. B. This descent of half a degree was graudal, but did not commence till long after the beginning of the experiment.—After this the piece of ice was inclined to one side, by which nearly one half of it was immersed in the cooling liquid, and the inclosed bulb of the thermometer was now not more than an inch from the cold mixture.

H.	M.	Therm. in the liquid.	Term. in the ice.
1	50†	14°	28°
2	20	19	28
2	50	22°	Ice along with the therm. slipped down into the cold liquid.

The ice now weighed 12½ ounces: the rest had been liquified by the operation of the saline liquor.

This experiment, I think, decidedly proves that ice is a worse conductor of heat than water:—Indeed this is not wonderful; for it is said, that ice at a low temperature becomes an electric.

It is certainly a remarkable circumstance, but not at all inconsistent with the known laws of heat, that in a mixture of hot and cold liquids, the uniform temperature should be so soon induced by agitation and so slowly by rest: But when we consider, that in the former case, hot and cold particles are brought together, and that in the latter there is a series of particles one upon another, gradually rising in temperature, but differing by insensible degrees, we shall not wonder at the facts. When any one particle of water, or any other body, has one above it, warmer by an insensible degree, and another below it, colder by an insensible degree, its power to transmit heat must be very small.—These considerations gave rise to the two following experiments.

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† From the beginning of the experiment.

Experiment 10.

A mercurial thermometer was taken, its bulb $\frac{4}{10}$ inch in diameter, and hanging clear of the scale: It was heated by the flame of a candle to 600°, and then laid upon a table with the bulb projecting over the edge, and was thus left to cool by the mere operation of the air in the room, which was 52°.—The following is the medium result of two experiments, which, however, agreed with each other almost in every observation.

Time.	*Temp.*	*Time.*	*Temp.*
0	600°	18 half m.	66°
1 half m.	450	19	64
2	350	20	62
3	280	21	60
4	229	22	59
5	195	23	58
6	168	24	57
7	145	25	56
8	128	26	55
9	115	27	54
10	104	28	53+
11	95	29	53
12	88	30	53
13	81	31	53
14	77	32	52+
15	73	33	52
16	69+	Air in the room	52
17	68		

Experiment 11.

Another thermometer, having a similar bulb, but a scale with much larger degrees, was heated and cooled in the same manner.

Time.	*Temp.*	*Time.*	*Temp.*
—	85°	8 half m.	$61°\frac{1}{2}$
1 half m.	$79\frac{1}{2}$	9	60
2	75	10	59
3	71+	11	$58\frac{3}{4}$
4	$68\frac{1}{2}$	12	58
5	$66\frac{1}{3}$	13	$57\frac{1}{4}$
6	$64\frac{1}{2}$	14	57
7	63	15	56.6

Time.	*Temp.*	*Time.*	*Temp.*
16	56°.3	24	55°·3
17	56¼	25	55·25
18	56	26	55·2—
19	55·9	27	55·1+
20	55·7+	28	55·1—
21	55·6+	29	55·+
22	55·5	30	55
23	55·4		

In these experiments we may consider mercury and air mixed together of unequal temperatures, with a thin partition of glass—and from the last we may conclude, that the thermometer imparted to the air 40 times more heat in half a minute, when its temperature was 30° above the air, than when it was only 1° above it.

We shall now advert a little to Count Rumford's experiments.—It will easily appear, that arguing fairly upon his own hypothesis he can never account for the phenomena observed: For, hot water being poured upon ice, an internal motion would take place near the surface of the ice, by which a stratum of water of a certain thickness would be reduced to 32°, and then all further reduction of the ice must cease; because all the superincumbent water being above 53° would be lighter and could not descend to the ice. But this is quite contrary to what took place. The facts, however, will admit of a satisfactory explanation upon established principles.

By experiments 10 and 11, it appears, that the quantity of heat given out by a body, during any small given portion of time, is nearly as the excess of the temperature of the body above the cooling medium. Hence, then, we may conclude, that the effect of hot water upon ice arising from the proper conducting power of water, will be nearly as the heat of the water. What effect the other cause may produce, it will be difficult to determine from theory: Experience will be the best guide. One thing, however, appears pretty certain, that its effect must be a *maximum*, when the temperature of the water at large is 42°½; because then there can never want a determination of the particles *downward* to supply the place of the lighter water of 32° ascending. If the temperature of the water exceed 42°½, then the effect of the internal motion will be less, diminishing by some unknown ratio. As far as I can judge from Count R.'s experiments, the joint effects of those two causes should be nearly the same with water of 42° and water of 190°. Taking this, therefore, for granted, we shall be enabled to sketch a table of the values of these two causes for every 10° of temperature. The numbers expressing the effect of the proper conducting power, are derived from the 10th experiment, and consequently are not purely hypothetical: those expressing the other effect, except 42° and 192°, are put down hypothetically, because the law of decrease has not been ascertained.

It is to be supposed, that a given quantity of water, of the several temperatures mentioned, is carefully poured upon a cake of ice at the bottom of a cylindrical glass jar, and stands without agitation for a given time, as half an hour; then the proportionate quantity of ice supposed to be melted by the two causes separately are stated in numbers, and then the sums are taken to express the joint effects.

Temperature of the water on the ice.	32°.	42°.	52°.	62°.	72°	82°.	92°.	102.°	112.°	122.°	132.°	142.°	152.°	162.°	172.°	182.°	192.°	202.°	212.°
Ice melted by the conducting power of the water	0.	2.	3.	6.	9.	11.	12.	14.	17.	19.	21.	23.	25.	27.	30.	33.	35.	38.	40.
Ice melted by the internal motion of the particles of water.	0.	36.	35.	31.	25.	19.	16.	14.	12.	10.	8.	7.	6.	5.	4.	3.	3.	3.	3.
Total quantity melted by both causes.	0.	38	38.	37.	34.	30.	28	28.	29	29.	29.	30.	31.	32.	34.	56.	38.	41.	43.

After what has been said, I need not caution my readers not to consider this table as accurate. The principle of it, however, cannot I conceive be disproved: that the operation of the conducting power must be proportionate to a series of numbers beginning from 0 at 32°, and gradually increasing in some ratio with the temperature above 32°, cannot, I think, be controverted; and that the operation of the internal motion must begin from 0 at 32°, and increase till it arrives at its *maximum* at 42°½, and then decrease again ever after, is also, I apprehend, unquestionable; thus, when the jar had water of 42°, in Count R.'s experiments, this internal motion must have had a range of 8 inches in depth; whereas, when hot water alone was used, it had not more than ¾ of an inch to range from the temperature of 32 to that of 53°.

The following table exhibits a concise view of all the material varieties of Count Rumford's experiments, with their result.

	Temperature of the water when poured on the ice.		Medium Temperature.	Water in the jar surrounded by		Ice melted in 30 minutes.
	At the beginning	end				
Experiment 32	41°	40°	40°½	Air	41°	617 Grains
35	189	180	184½	{ *a warm covering of cotton wool.* }		747
38	41	43	42	Air	61	676
39	180	157	171½	Air	61	559
45	188	64	128	Ice & water	32	406
51	61	49	55	Ice & water	32	660
53	61	60	60½	Air	61	642

Count Rumford attempts to explain why there was less ice melted in such experiments as the 45th than in those like the 39th, and attributes the diminution of the effect to the descending currents, occasioned by the cold mixture surrounding the warm one, which he thinks would obstruct the opposite ones ascending from the ice. But the effect in the 51st, compared with the 53d, being just opposite, he passes over without explanation.—I have no doubt myself, but that the true cause of the differences in both cases, is to be found in the column expressing the *mean temperature* of the water, and not in that expressing its situation, which I consider as having nothing to do in the business, but as it affects the general temperature. The *maximum* effect with cold water will be when it is of the temperature of about 48° or 50°, and the *minimum* above it probably about 100° or 120°; and in proportion as the mean temperatures, in any experiment, deviate from those points, the effects vary accordingly, let other circumstances be what they may.

Thus I have attempted to explain the rationale of these very curious and interesting experiments, in a manner different to what their ingenious author has done. And must now leave it to the reader to form his opinion.

EXPERIMENTS *on the* VELOCITY *of* AIR *issuing out of a Vessel in different circumstances; with the Description of an instrument to measure the force of the Blast in Bellows, &c.* *By* Mr. BANKS, *Lecturer in Natural Philosophy. Communicated by* Mr. DALTON.

READ, MAY 30, 1800.

THE object of this enquiry may be announced in the following proposition. If an elastic fluid is generated in a given vessel, or any way contained in it, and at liberty to issue out of the said vessel through a given aperture, to determine the resistance which the vessel meets with from its action, or the power which it has of communicating motion to the vessel, as in a sky rocket, Saddler's steam-engine, &c.

Before we proceed to relate the experiments it may be proper to premise certain principles deduced from Theory. If a tube be filled with any kind of fluid, as *air*, *water*, *mercury*, &c., and placed in a vacuum, every fluid will flow out with the same velocity. For though the pressure of a column of mercury of a given altitude, be much greater than an equal column of water, yet the weight of the particles to be projected is greater in the same ratio. On the other hand, if air is lighter than water, the particles projected are also lighter in proportion. If a tube 16 feet high be filled with air of any density, that air, like water, would flow into a vacuum with a velocity of 32 feet per second, no corrections being made for resistance.*

And if we take the gravity of air to water as 1 to 840, then a column of one foot of water, compressing air, will produce as great a velocity in that air as a column of air 840 feet high, supposing it was of uniform density.

If we take the whole pressure of the atmosphere equal to 33 feet of water, or its height (supposing it to be equally dense, which in this case will make no difference) equal to 33 multiplied by 840, or 27720 feet. Then as the square root of 16, is to 32, the velocity at that depth; so is the square root of 27720, to 1332 feet per second, the initial velocity of the atmosphere into a vacuum.

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* In the supposition of a perpendicular tube open at the top, filled with air or any elastic fluid, the author takes the density of the column at the bottom or where the aperture is made, to arise solely from the weight of the elastic column; and the altitude to be that which would be if the whole column were reduced to the density of that at the bottom.

To prove whether air compressed by 33 feet of water would be impelled into the atmosphere with the above velocity, I have made, amongst many more, the following experiments.

A is a vessel of a known capacity, into the top of which is screwed an aperture of a known area. The tube *T d* recurve at *d* is soldered or screwed into the top of the said vessel. The hole *a* is stopped, and water poured into the tube at *T* till it is full, at which time a quantity of water will have passed out of the tube at *d*, and condensed the air in the vessel, more or less as the tube *T d* is longer or shorter.

At this time a person who has closed the aperture at *a* with a finger of one hand, and held a half second pendulum in the other, removes both at the same time, while at the same moment an assistant opens a cock over the tube *T*, which supplies it with water as fast as it can descend into *A*. The moment that the water appears at *a*, the time piece is stopped, and the time of expelling the air is noted, from which, by knowing the capacity of the vessel, the velocity may be obtained.

If the tube *T d* should be continued near the bottom of the vessel *A*, while it was filling with water, the length of the compressing column would be gradually diminishing, and of consequence the pressure would be constantly changing, hence the open end of the tube is as near the top of the vessel as is consistent with a free passage for the water,

Experiments.

The vessel *A* contained 15lb. 6 oz. of water, from which we find its capacity is 425.088 cubic inches.

The area of the aperture *a*, through which the air is expelled is ,0046 inches.

Exper. 1st. The altitude of *T* above the vessel is 30 inches. Time of expelling the air by several trials is 33 seconds.

Exper. 2d. The altitude of *T* is 6 feet. The time of filling by several trials is 21.3 seconds.

In the first experiment, 425.088, the solidity of the vessel, divided by ,0046, the area of the hole *a*, gives 92410.4 inches for the length of the stream of air driven out in 33 seconds; divide that length by 33, and we shall have 233.3 feet, the velocity per second, communicated by 30 inches of water, The second experiment by the same process gives 361.6 feet per second. If we would compare these together, we may say, as the square root of 30, the head, is to 233.3 the velocity; so is the square root of 72, the second head, to 361.8 feet the velocity per second.

Again:—As the square root of 6 feet, is to 361,6; so is the square root of 33 feet, to 845.2 feet per second, the velocity produced by that head; or the initial velocity with which the atmosphere would enter a vacuum. This velocity found by experiment is 487 feet per second less than has been assigned by theory.

It appears however that the results as determined by theory and experiment do not differ more than in the case of effluent water. For, if we would reduce the velocity of effluent water, found by theory to that which experience gives, we must multiply it by ,634. Accordingly, if we multiply 1332 feet, the velocity of the atmosphere entering a vacuum, as calculated above by ,634, the product is 844.5 per second, differing but $\frac{7}{10}$ of a foot from that just found by experiment.

I have also made experiments by sinking vessels in water, till their tops were even with its surface, and opening the aperture that the rising water might expel the air, by which I obtained the same velocities as above; but the method of computing is much more intricate, for which reason I shall not insert them.

From the above, it appears that a pressure equal to 33 feet of water, will expel air out of bellows into the atmosphere with a velocity of 845 feet per second, that *one foot* of water in depth will produce a velocity of 147¼ feet, and *one inch* a velocity of 42 feet per second, or 20 miles an hour.

Hence we may construct a table shewing the velocity communicated to air by any head of water. For, as the square root of 6 feet, is to the velocity produced by that head; so is the square root of any other depth, to the velocity produced by that depth.

We may also, from the above, construct an instrument which will shew the velocity with which air flows out of any kind of bellows, with as much accuracy as the experiments have been made, on which its construction depends.

Description of the Instrument, &c.

The metal box or tube *B*, may be about the size of the figure; the top must be made air tight by the cover *L*; into the bottom is fixed the small tube *AC*, and into the piece *D* is cemented the glass tube *ED*; the instrument is then inverted, and some water poured through the tube *AC*, till when in its proper position it is visible at *D*. It is now ready for use, and the end *A* may be fixed in a hole made in the upper board of the bellows, and the water will rise in the glass tube, in smiths' bellows generally from 9 to 12 inches, furnace bellows generally 4 feet or more. But where the compression is great, quicksilver may be used instead of water, only in this case the instrument should be made of iron, as quicksilver causes the screws of brass to break. Or, instead of quicksilver, the tube *ED* may be sealed at the top *E*, and then a length of 12 or 18 inches will be enough for any blast. The glass tube needs not be more than one eighth or one tenth of an inch in diameter.

Whatever compression there may be in the bellows there will be the same in the upper part of the tube *B*, which will force the water into the glass tube *DE*, and make the air in its upper part of the same density, deducting from the compressing force, the altitude of the water raised above *D*, which however will be of little or no importance; if the gauge is placed in a horizontal position, with the glass tube downward, there will be no difference of density.

The computation for the force in the case where a tube hermetically sealed at the top is adopted in the instrument, will be effected by considering that the space occupied by any elastic fluid is inversely as its force.—Thus, let the tube be 12 inches long, and suppose the water to be raised 1 inch; then it will be 11 : 12 :: the force of the atmosphere : the force of the air in the tube :: 1 : $1\frac{1}{11}$.—Hence a scale may be adapted to the instrument, to express the force of condensation over and above the common atmospheric pressure; which force is signified in the instance above by the fraction $\frac{1}{11}$, unity being the atmospheric pressure. If we denote the atmospheric pressure by 30 inches of mercury, or 32 feet of water, then the force $\frac{1}{11}$, in the above example will be expressed by 2,727 inches of mercury, or 2.91 feet of water; and the like for any other instance.

If a mercurial instrument of the above construction be preferred, it becomes necessary to add the height of the mercurial column to the force found as above: thus, if the condensation of air be from 12 into 9 inches, then the addition to the force of the internal air in the tube is equal $\frac{1}{3}$, or 10 inches of mercury, to which must be added the 3 inches raised in the tube, and the whole force will be 13 inches of mercury, exclusive of that of the atmosphere.

This sort of instrument or guage serves equally well for finding the expansive force of any kind of elastic fluid, as for measuring the velocities with which they issue out of the place of their confinement. It may be applied to all kinds of bellows, to condensed steam, and to the air pump.

ESSAY *on the* BEAUTIFUL *in the Human Form; and Enquiry whether the Grecian Statues present the most perfect* BEAUTY *of* FORM, *that we at present have any Acquaintance with.*

Communicated to the Society from a CORRESPONDENT, through the REV. GEORGE WALKER.

READ, Oct. 3, 1800.

In order to judge whether the Grecian imitations, or any other imitations, of the Human Form be the most beautiful, it appears necessary that some standard, some general law or rule should be admitted, in conformity to, or in deviation from which, the sentence of beauty or deformity may be safely passed. That this standard has an existence in nature can hardly be doubted; for, if man be the work of a designing artist, he must have been formed according to some model; and this model in the contemplation of the artist must be the standard of what is the most perfect of the species, and as far as the form is concerned, of what is the most beautiful in the form. The mind of the artist may then be investigated in his work, and it might seem to be no very difficult thing to collect a tolerably accurate idea of what answers to the more perfect idea of the artist, by omitting what is incidental and peculiar to every individual of the species, and retaining what is universal. And perhaps by a standard thus collected, though insensibly, and without any deliberate purpose, every one does judge of what he deems to be beautiful in the form of his own kind, and in every form whatever. We find therefore that, in the estimation of the beautiful in the human form, there is a general agreement as to the contour or outline of the whole and of the parts, the comparative magnitude of each part, the proportion that each bears to each other and to the whole, and the order and degree in which each swells and falls. Whatever is remarkably excessive or defective, whatever strikingly offends against the general character and proportion of the parts, whatever beyond the general rule is abrupt and extravagant in the swell or fall, is almost universally rejected as not beautiful;

because not answering to that medium standard which every one has erected in his own mind, and which he has collected from the exhibition of his species. There is therefore in the imagination of every one a standard, collected from observation, but insensibly and without design, to which he refers every form, that attracts his attention, and agreeably to which he pronounces that it is beautiful or otherwise, and in what degree it pleases or offends. It is an ideal figure, which the eye of the mind can contemplate, and does contemplate and does refer to, though the rational mind cannot describe this figure; because the figure has been imperceptibly formed, corrected, improved through life, in which the senses, and not reason, have been altogether employed. The ultimate figure, as a picture of the imagination, is the abstract of all the impressions which have been received from a multitude of original forms; preserving what is characteristic of all, and rejecting whatever is incidental, excessive or defective, superinduced by violence or art, or in any respect offending against the general character of the form.

This perhaps is the secret foundation of what we call taste, or the perception of the beautiful, whether as referred to the human or to any other form whatever. I do not say, that this is the only foundation, and that there is no other rule or principle, by which our estimation of the beautiful is influenced; but I think it to be the principal foundation: and the remarkable consentaneity of taste and decision of the beautiful, especially of the human figure, proves that our rule is derived from nature, from our constant observation of the originals, as they have issued from the forming hand of the great artist; and that thus acquiring an abstract idea of the whole, we enter as it were, whether intending it or no, into the mind of the artist himself, and erect a standard of what he designed as the most perfect, and therefore the most beautiful, in the form which he has given to man.

In this investigation of the standard of taste and the decision of the beautiful, although a general consent be acknowledged, yet it is manifest that the rule or standard will be more or less perfect, as the field of observation has been more or less extensive, and as successive comparisons with the standard already attained, and the introduction to more perfect forms, with fewer deviations from the medium character, have chastened in the imagination the picture of the beautiful. Every step in the progress towards this perfect image is the selection of a few from the general mass; from continued observation rejecting some from this few; admitting others; till the field of observation is exhausted, and we rest in the image which is the result of the whole.

But as the originals which are the subject of observation will in some respects vary from the influence which climate, occupation, manners, and even the cultivation of mind, have on the human form, it will follow that a variation in the idea of the beautiful is to be expected, and that different circumstances may be so favourable to some, as to render their conception of the beautiful more approximate to the faultless truth and standard of nature.

This secret and imperceptible progress towards an ideal standard of beauty may be illustrated by the supposition of an experiment, easily to be conceived, though not easily to be carried into execution. If impressions from the faces of all the women in this kingdom at the age of twenty, were taken on any plastic substance, as suppose plaster of Paris; excluding however those who come into the world with obvious excess or defect, who have been maimed by injury, or blemished by any superinduced cause, as excess of labour or rest, intemperance, deficiency of sustenance, or any excess or defect of the passions of the mind; and an artist were to form a face that was the mean of all these; it would surely be admitted that this face would be the perfect model of our national beauty. If the same experiment were made in other nations; excluding those in whom the extremes of climate necessarily induce a depravation of the form, the model of beauty in the female face would be equally obtained in these nations as their appropriate standard. And if from these several national standards the mean of them should also be taken, this last image must be admitted to be as perfect a representative of the beauty of face, of the whole female race, as is possible to be obtained.

This judgment is founded on the supposition that the design of the Creator is evidenced in his productions, and that the mean character of his productions of any species must approach the nearest to the perfect model contemplated in the Creator's mind. Our sense of beauty, our delight therein, can find its object only in the production of the Creator, our taste conforms itself to what is done, and the mean character must be the highest standard, the utmost conception of beauty that we can form. Either our ideal standard or image of beauty is born with us, or it is thus acquired; the former would be a mere arbitrary supposition, the latter accords with experience, is confirmed by reflection on our daily experience, it is the necessary consequence of every moment's impression from the objects presented to our view; and if our minds were not insensibly led to this result by being committed to the field around us, yet, as the means to the end, it might in this very way be accomplished by our deliberate act, by experiments similar to what I have supposed, in collecting the standard of national female beauty at the age of twenty one.

It might be expected therefore, that the standard of beauty would be one and the same to all; and so it is, as far as one and the same rule and judgment on any subject can be expected in the vast range and diversity of human beings. It has already been observed that the greater is the variety and number of the objects that have been viewed, and attentively viewed by any individual, the nearer will the standard of beauty in his mind conform to the mean character of the species, which has been submitted to his view, and probably therefore to the truth of nature.

To a perfect uniformity in the judgment of beauty it is requisite that the field of observation be equal to every one, that the mind of each observer be equally directed and equally attentive to the subject, that the circumstances which are favourable or unfavourable to beauty should be equal in the objects observed. But this equality has no existence, and therefore smaller variations in the estimation of beauty are certainly found; but withal, there is so much of consentaneity in the opinion of all mankind, as proves that the standard in every mind is derived from a common source, and has much of a common character. I can have no doubt that a Grecian Beauty among the Greeks, a Circassian Beauty honoured by the admiration of the Asiatics, would be acknowledged as a beauty of high distinction by the western European, and that the European female whom the European taste has selected would be in no small degree applauded by the Greek and Asiatic. Perhaps, even a first rate African in the estimation of her fellow Africans would be allowed by the European, the Greek and Asiatic, excepting colour, to possess the essential characters of beauty.

It is probable therefore that the idea of beauty, though acquired by observation only of the examples presented to our view, goes beyond the limit of the materials from which it is derived, and is more perfect than the mean character of the objects observed. In acquiring the idea of this mean character we reject all obvious excess-

or defect; we obtain thereby an abstract idea of excess or defect; we admit this as still in some degree adhering even to the most perfect originals that we have seen; we conceive that still more faultless examples may be found in the vast field of human life; and our imagination creates to itself a more perfect idea than our experience has presented.

The result of the whole is this, every one has within him a standard of beauty, and this standard is acquired by every one from the same source, and is in all of a common character; but will be more perfect, that is, more conformable to the truth of nature, as the field of observation has been more varied and extensive, as the attention has been more excited and directed to the investigation of beauty, and as the objects of observation have been in those circumstances, which are most favourable to the preservation and perfection of the human form, and wherein the original stamp and design of nature may be, either not at all, or in the least degree, counteracted and injured by adventitious causes. I pretend not to judge, whether in this investigation of the standard of beauty there be any thing of novelty or ingenuity; it will be sufficient to my purpose, if it be agreeable to truth, and be pertinent to the second object of this essay; and support the claim, which I assert for the Greeks, that in their statues are found the most perfect imitations of the beautiful in the human form, that have been introduced to our knowledge. If no proper standard of beauty can be acquired in any other way; if the Greeks perfected their conception of the beautiful by a nice and delicate attention to this rule; if they had more favoured originals to collect from, and had a freer access to these rich originals; if the Greek statuarists were men of the first consideration, and besides their special art, had their minds raised and chastened by all the advantages, which learning, science and cultivated taste can confer; then it is a well-founded presumption, that their statues as imitations of the beautiful, are of the first form, and have probably not been equalled by the similar productions of any other nation, if to the statuaries of no other nation the same means and advantages have been extended.

This standard of beauty thus acquired, and the only standard common to all men, may be called the Sentimental standard; as its primary derivation is from the immediate sense or impression of the human form upon the mind of the observer, and its ultimate result is the mean of all the agreeable sensations which the beautiful of human form have made upon the mind.

But there is also a Rational standard, which more contemplative and reflecting minds derive from a consideration of the uses to which the human form subserves. Beauty, though in itself an object of regard distinct from every other consideration, must, in the productions of every wise artist, be subordinate to utility; and therefore in those works, which we refer to the Great Artist, we expect that beauty shall be reconciled with utility.. Nor are we disappointed; all that is beautiful in the human form, which we contemplate with so much delight, is, as a mean, in perfect harmony with the numerous and diversified uses for which the human form was designed by its Creator.

Beauty could not be a primary object in the mind of the original artist, nor are we authorized perhaps to say that in our sense of beauty it was any object at all. When this artist designed man, *that* must to him have appeared to be the best form, which was the best fitted to the field of action in which man was intended to move, and in which it was intended that he should reap the conveniences and utilities of his being. If man had been designed to be an animal of speed, a form similar to that of the hare, of the greyhound or the antelope might have been assigned to him; or if not precisely a similar form, yet in the limbs adapted to motion, a lightness of bone, and firmness of muscle similar to what characterises these animals. And by parity of reason, the qualities of the ox or elephant, if slowness of motion united with great capacity of burthen had been designed to be his predominant character; or if a temperament of speed with burthen, then a tempered union of the forms which distinguish the hare and the ox, as in the horse, might have been the character of the human form. But as each of these uses in a very limited degree, and subservient to many other higher uses; such as quickness of movement towards every part; the *os sublimé* to contemplate heaven as well as earth, and each with the greatest range; exquisite sensibility, particularly in the hands, which are the instruments of mechanic operations; and in fine, the unnumbered and varied uses, which the inventive and creating mind of man can meditate, were intended to constitute the character of man, we find therefore that a form is assigned to him, which is adapted to all these uses, and to each in that degree in which it contributes to the concentered and harmonious utility of his whole being. But I mention not this, with any of the views of the naturalist or divine, but merely to shew that man can in some degree enter into the mind of his Creator, and, pleased with his form, pronounce also that it is good; and believe that it is beautiful, because it is eminently useful; and as, without any consideration of utility, he pronounces that to be the most beautiful of all the varieties of the human form, which is the mean of them all, so infer the same conclusion, when he estimates the mean of all utilities, regard being had to the respective worth and dignity of these utilities.

It is therefore more than probable, that whether we estimate beauty by the mean of utility, or simply by the mean of form, we shall arrive at the same, or nearly the same conclusion; we shall attain the same standard. But the investigation of these utilities in all their comprehension and reference to their best result is more difficult, and except in obvious and familiar instances, does not excite attention in common observers. This common sense of man however in the most familiar instances is an evidence of vast import, the mind expects and is reconciled to the more delicate judgments of more attentive observers; and a persuasion is induced, that if man were competent to the complete analysis of himself, he would find that the mean of his whole form is admirably adapted to the mean of all his utilities.

It is surely agreeable that the standard of beauty derived from two very different sources should be found to be one and the same. In receiving that as beautiful which is, we defer to the divine wisdom; in the appeal to the reason of man, the mind and act of Deity is justified to the mind of man.

But there is also a use in referring the judgment of beauty to these two tests, as they mutually serve to obviate some difficulties, to reply to some objections, and to correct some erroneous judgments to which we might be liable, if we had only one test to appeal to. The expectation of the beautiful, according to the sentimental standard, in all of the human kind, is repelled when we take into our view the varieties of human life, and admit that these varieties require a conformation nicely adapted to the useful in all. Whether it be that difference of climate, of situation, of employment bodily or mental, and of age, induce, as the mere effect of a cause, such smaller variations of the human form; or that man generally issues from the hand of his Maker with such variations of form as may furnish all the diversified agents fitted to bear their several parts in the vast community of men, makes no difference in the conclusion. Wherever the diversities of the useful require a deviation from the absolute standard of sentimental beauty, we submit our judgment thereto, and acknowledge a specific beauty; and with delight acknowledge, where adaptation to strength, to toil, to hardihood, to boldness, to enterprise, to speed, to versatility or any other peculiar end, is the predominating character of the form. In the distribution of men therefore into classes according to their situations and subservience in life, the rational standard admits a beauty, peculiar and appropriate to each; which if not found would be deemed to be a defect, though the degree in which this specific character is required would not be admitted either in the rational or sentimental standard of the mean perfect beauty of the whole human kind. Thus a length of arm, and lightness of leg are well adapted to the maneuvres of the sailor, but would be unsuited to the pedestrian, in whom is required a strong and muscular leg with as little super-incumbent weight of body as is consistent with general vigour. And thus in the different occupations of men, the estimated beauty of form in each class will be adapted to the part which each has to sustain in life. A general, when he looks with pride on the manly figure of his soldiers, has a different standard of beauty in his eye from that by which he would judge of the graceful and elegant in an assembly of the higher ranks of life. We observe a similar rule in the judgment we form of the different classes of animals. The beautiful in the lion, the tyger, the elephant, the bull, the horse, and the dog, is referred to a different standard in each, and even in those species, which come more under the observation and use of man, as they are subdivided into different kinds, they have each their appropriate destination and appropriate character, and the mean form in each subordinate kind is allowed to be the standard of beauty in that kind, though varying from the standard of beauty in each other kind, and varying from the standard of the beautiful in the whole genus, where an aptitude to a particular destination is not contemplated. The dray, the road, the race, the field of chace and the field of war have each their proper beautiful of form, while each partakes of those qualities, which enter into the mean beautiful of them all. And the same is observed of the subdivisions of the canine race; the shepherd's dog, the terrier, the spaniel, the fox-hound, the greyhound, the bull-dog and the mastiff, to which we may add the favourites of the ladies, are all estimated by very different standards of beautiful form, and yet are all referable to a form which is the mean of them all, and is conceived to be the most perfect idea of beauty in the genus.

This attention to the useful, and a fitness for the part which each is intended to act in the plan of their Creator, is strikingly witnessed in the idea which every one entertains of what is beautiful in the form of each of the two sexes of the human race. No one conceives the beautiful in the man to be exactly transferable to the beautiful in the woman, and if the most perfect beauty of the female were found in a male, the eye would be disgusted; that male would be the object of scorn or ridicule, not of approbation.

Perhaps the mere abstract idea of beautiful form, independant of every other consideration, would pronounce the gentler undulations of the masculine outline to be more conformed to its standard, than the richer swell and deeper falls of the female figure. Yet in despite of this abstract judgment, the most perfect masculine figure, if contemplated in a female, would not be considered as beautiful.

The same submission to the useful, or to what we presume to be useful, because it is according to the order of nature, is observed in the estimation of beauty, as it is referred to the different periods of human life. There is a different beauty in infancy, in boy-hood, in youth, in manhood, and in age. The child in the arms of the Madonna, and John attracting the notice of his infant cousin by his playful admiration, though the difference of their age be but very inconsiderable, yet would each, independant of all regard to the part which each is designed to sustain in riper years, be expected to exhibit a difference of form and aspect. In the full-blown rose of Lucretia, and the opening bud of Virginia, which equally enflamed the passions of Tarquin and the Decemvir, and led them to the perpetration of the most daring and dangerous crimes, though in each we imagine something that answers to our perfect idea of female beauty, yet in each we look for a delicate discrimination of exterior, something appropriate of form and feature, expressive of their respective age and situation.

In the domestic groupe of mother, wife, children and attendants, which issued from Rome to avert the vengeance of Coriolanus from his country; if imagination give a beautiful figure to each, it will be a beauty proper to the age of each, and proper also to the relation which each bears to the avenging hero, and to the shades of affection and interest with which each must be impressed from all the circumstances of the interview; while in Coriolanus himself, that which we should acknowledge as beautiful and proper in his figure would very materially differ from that of all the preceding.

There is therefore indubitably, as is generally allowed, a peculiar beauty of form and feature adapted to each of the passions; but here also, in order to the perfect, the mean must be observed; and this perfect form, as expressive of the passions, rejects the predominance of any one feature, which usurps over the others, which subdues the whole form and countenance to one characteristic and generally disagreeable expression. This must enter in no small degree into all our judgment of the human figure; the sense both of the beautiful and the proper must associate with our conceptions of the mind, which the configuration of the form in all its parts, and in all the acting of mind thereon, is fitted to express. In every human figure, which on any account attracts our attention, the immediate impulse is to read the mind in the face. Animals read it; it is an universal character, and designed to be read in some degree by all who have an interest in its information; for it is the hand-writing of the Almighty, and inasmuch as mind is superior to body, that form is pronounced to be more beautiful, which in every expression presents a beautiful mind, and at the same time is conformed to the interesting affections which are proper to the varied situation and circumstance of mind. Perfect beauty therefore supposes perfect beauty of mind, and deformity in both is the violation of the mean. Perfect mind admits of no predominant passion but benevolence, but even benevolence itself would be tame and insipid, if it excluded every other passion. It would not be suited to the field of human action and human sympathy, and could not invite our sympathy in return. The only influence of passion that can be delineated with an attention to perfect beauty is that which admits the expression of every passion; but of each according to its worth and dignity, and the power which each in its place and rank ought to have over the form and countenance; and that face which bears the expression of this temperament and proportion will be, so far as respects the influence of mind, the most beautiful.

But even in the expression of the passions, the male and female standard will differ, nor can the temperament required in the one, be expected in the other. That mildness and placidness of countenance, which is the lovely picture of a gentle female mind, would not give the promise of a masculine mind, nor answer to our standard of masculine beauty; and though the morality of mind will not submit her rule to any examples of human life, and will without hesitation pronounce that countenance and figure to be deformed, on which the violent and intemperate passions have stamped their character; yet in the imitations of man as he is, the very imitation will soften the deformity, and render that not only acceptable, but approved, and as a beauty in the art, which we should fly from with disgust and horror in the real object.

So far then we have proceeded in the examination of the beautiful, not merely as a prelude to our deciding on the excellence of the Grecian statues, though even as a prelude this appeared necessary; for without some rule founded in nature, all decision on the beautiful in the imitative arts of the Greeks, might be considered as mere opinion. If we have not erred then in this previous analysis, it has appeared that, in the subject of the human form, the sentimental and the rational judgment of beauty are not discordant, but that the beautiful as a matter of feeling harmonizes with the useful as the end; and as an influence from this, that the perfection of beauty as an absolute standard must be looked for in the middle form of the whole collected human race, excluding all whom accident, violence, monstrous births, or the extremes of climate, food and labour may have disfigured; and also in the middle form between the extremes of age, in which the subject has not attained to or declined from the point of perfection, because all the varieties of the human form seem to respect this medium both of number and age. It has also appeared, that in the distribution of occupation and character, and in the expression of the passions, that interesting language of the human form and ſeature, there are separate standards, which have each a specific beauty as their appropriate character, though subordinate to the more universal standard, and acknowledging it as their genus. Not greatly deviating from this higher standard, each of these distributions must be distinguished by their characteristic form, or they would not be beautiful in their place.

Having therefore to the best of my ability laid this foundation, I shall proceed

II. To enquire whether those statues which have been preserved to us as monuments of Grecian art and taste do correspond, and in what degree, to those high conceptions of beauty in the human form, which both from sentiment and reason we are led to entertain.

Appeal then to the sentimental standard, that is, to the most abstract idea of simple beauty, without any consideration of utility, or destination to particular purposes and offices in the distribution of human action. Submit to the eye the best of the Grecian Statues of this character, submit them to the severest examination, place them before the ideal standard in the mind, and say whether imagination can figure to itself more exquisitely finished models of perfect human form. Can the most fastidious critic say that this part is too large, that too small, that the wave of the contour might be more happily exhibited. That timidity wherewith nature fears to exceed, when she delights to present to us the perfect of beauty, is delicately charactered; every thing is in that happy medium, that mild temperament and proportion which perfect beauty requires; no muscle harshly obtrudes itself to break the charming roundness of the form; the minutest parts, the joints, the fingers, the toes, the very dimple, as impressed by the playful finger of love, is touched in the truth of nature; in fine, the eye of the delighted spectator may look for hours and days, and not dare to say, not be inclined to say, in what respect they could be altered, so as to be rendered more grateful to him. The eye is not satiated with viewing them, they strike not at first with their full impression, but every return to them makes a deeper impression, unfolds new beauties, discovers the grace of nature in her most finished works, and this perhaps is the most decisive proof of their superior excellence. If any thing be wanting, it is colour and the expression of the eye, but of these stone and marble are incapable; the artist has done his part, he has effected all that the material was susceptible of. Perhaps it is owing to this, the impossibility of communicating to stone the soul which speaks in the countenance, that the face of the

Venus de Medici answers not to our expectation, that it appears to be not so beautiful as the whole and every other part of the form. But the artist has given to the whole attitude the expression of apprehensive modesty, and of the will to move, you expect the statue to step from the pedestal. In the Apollo Belvidere is presented every thing that elegance and grace with a chastened severity and dignity can require in a perfect masculine form. The beauty of Antinous, the Catamite of Hadrian, is feminine, but the form of Apollo is such as mind giving its character to form and countenance would prepare you to expect. He is not Jupiter, nor Mars, nor Bacchus; there is blended in his figure the excellence of each, but with no extreme; he is such as you would look for in the ruler of the day, the president of the muses, in the handsomest and the wisest of the gods.

This delight, which the Grecian antiques excite, and as a matter of feeling, without any philosophical enquiry, has been witnessed by all, in various ages and nations. The Greeks copied from nature, her most perfect forms were their standard; succeeding artists have received their productions as a standard, and looked for reputation as they approximated to the perfection of their works. The antient Romans in the highest pride of their empire deemed nothing excellent in the art of statuary but what was of Greek fabrication. Their admiration knew no bounds; and though of the statues preserved to us, many are of an age posterior to the earlier Cæsars, it is probable that they were all the workmanship of Grecian genius. The school, denominated the Roman, though the Roman empire had been long extinct, was formed on the basis of Greek art; it aspired to no higher honor than to be the imitator of the antient Greek; yet inimical as imitation is supposed to be to great exertion, this school is allowed to have surpassed all other moderns in the beauty and character of its figures. While other subsequent schools of statuary, which have wrought from their own ideas, and been laborious enough in the execution, have each exhibited less beauty, less correctness, less conformity to what every one feels to be *το καλον* in the original. This concurrent testimony may almost be allowed to be decisive. Among the artists of Italy, and succeeding to them, in other countries, there are who appear to have devoted their whole lives to the study of whatever is most beautiful in nature or in art; and if their unqualified deference to the Greek be not admitted, I wonder from whom, with a more refined and better furnished mind, we may expect a juster sentence!

The artists of later days ascend no higher in the pursuit of the beautiful than the models which Greece has transmitted to us. Have any of them retraced the steps of the Greek artist in the formation of one celebrated statue? Have they selected from individuals of approved form, whatever is most beautiful in each, and thus composed a more perfect whole. Until this be done, it is mere presumption to suppose that they will surpass the Greek, that with all the aid of the antients they will produce a more perfect model of beauty, that this second extract of the beautiful would more answer to our most perfect idea from nature.

There is reason to believe that the Grecian artists did select the most beautiful originals, and from what appeared to be most perfect in each, formed their mean figure of the beautiful. It was, inasmuch as art could execute, the summary of what nature had dispersed in a variety of the best chosen subjects.

It is farther to be observed, and, if true, very important to the purpose, that no climate, no state of society, no modes of life and manners, were ever more happily adapted to the preservation of beauty, to the production of the most beautiful and most perfect designs of nature. A happy temperament of elements, inviting to enjoyment, and also to exercise, activity and sportiveness, simplicity of diet and simplicity of manners, were eminently favourable to the primary production, and to the preservation of beautiful form. The concurrence of these advantages is so powerful as to resist the barbarism and oppression of the Turkish government, and even to this day preserve to the native Greek race their pre-eminence of beauty.

The Grecian games invited the most perfect of the species, in varied character and exhibition, with all the advantage of exposure, and with all the action proper either to strength or grace; which must have furnished to the Grecian artist models of perfection in nature, and of the truth of nature in her best specimens, such as modern artists can have no access to. Our modern manners admit not of such exhibitions, and if in a moral view we have gained thereby, the loss to the imitative arts of sculpture and painting is incalculable. Nor, if it were otherwise, is it probable that in our climates and with our modes of life, any such specimens of the beautiful working of nature can be furnished. It will be allowed at least, that the hirelings, which are exhibited in our modern academies, are no substitute for the ampler and richer display of form and character, from which the Grecian sculptor and painter copied their admired productions.

The freedom of the Grecian mind must also have had a considerable influence on the feature and form.' The influence of the mind on the feature, where the soul speaks in all its energies and character and dominant affections, is acknowledged by all; but it is not equally considered to what degree the mind, cherished from infancy in all that generous freedom, which is the gift of its author, may determine the form, and rear it up in the grace and elegance and beauty which answers to the best intention of nature. The easy, flowing dress of the Greeks corresponded with this freedom of mind; whatever nature designed, she freely operated; no restraint forced her into awkward, ill-proportioned and ungraceful deviations; and what freedom of mind, ease of dress, salubrity of climate, and simplicity of diet and manners, left unfinished, their gymnastic exercises compleated. In modern Europe every thing almost is adverse to the production and preservation of beautiful form; mind is not so pure and unadulterated; modes of life are not so equal, nor so conformed to simple elegance; the form of the great mass of the community is as much injured by excess of labour, depression of mind, and exposure to unequal climate, with scantiness of food, or irregular supplies of it, and not of a simple and salubrious kind, as the form of the higher ranks is impaired by excess of food, equally insalubrious; exclusion from air and exercise; manners that awake no mental energies, invite to no pleasant, healthful, sportiveness; intemperance in the hours of rest; and, what alone is sufficient to every depredation of form and beauty, confinement in the poisoned vapour of crowded and heated rooms. The inference is obvious. The modern European cannot rival the artist of antient Greece. He has not the same originals. Nature presented herself unviolated to the Greek; injured and perverted, she can exhibit to the European only her weaker productions.

To these considerations may be added, what I have already alluded to, that the Greek artists were men of the first form, well educated, and of high consideration. Superior instruction, and admission to the highest honours, elevate the mind, excite grander conceptions, and exalt the taste; especially in an age and country, where the passion for fame was the *stimulant* to all great exertions, and furnished to every one the most generous gratification. The philosophic Socrates, that wondrous man among the Greeks, was himself a statuary, and is said to have sculptured three very beautiful figures of the Graces. Without intending any thing unhandsome to later artists, the same elevation of mind cannot be equally affirmed of them. The motive of gain, always sordid and depressing, and equally accessible to the lowest minds, has too much usurped over the more generous one of fame.

I shall conclude this essay, which perhaps is already too long, with another very powerful argument in favour of Grecian art; which may be inferred from the great length of time that it flourished, and the innumerable productions which it furnished. This argued a degree of fame attached to it, and an encouragement to rivalship, of which we have no example. We may judge of this from the immense number of works which escaped the repeated plunder of the Romans; and from the valuable remnant, which to our day has survived the destruction of ages, of successive revolutions, and the rudest barbarism. Memmius, Æmilius Verres, and Proconsuls, and Prætors and Generals, and Romans of rank and taste, beyond all calculable amount, might have been thought to have exhausted Greece of her rich treasures of art; but succeeding to them Tiberius Nero carried off a valuable plunder from the Acropolis, Delphi and Olympia, and yet in these very places not fewer than three thousand statues were remaining in the time of Pliny. Can any thing in modern times compare to this? Can modern artists recur to so grand a feast of the senses, so glorious a school for instruction in their art? Does the patronage of later times present any thing like such a provocative to genius and rivalship, as we may presume to have been the character of Greek and Roman antiquity?

A Defence of LEARNING *and the* ARTS, *against some Charges of* ROUSSEAU. *In two Essays. By the Rev.* G. WALKER, *F. R. S.*

ESSAY I.

(READ NOV. 15, 1799.)

That learning is not the parent of politeness, nor chargeable with the duplicity fraud and vice, which he supposes to be her attendants.

IT is a failing, and not of common minds alone, who surrender themselves to the impression of the moment, but also of men, from whom a more just appreciation of the past and the present might be expected, to indulge to a spirit of discontent whenever they speak of their own times; and with a kind of holy veneration to fix their eye on those days of old, wherein, as they suppose, ingenuous virtue and sincere enjoyment were alone to be found. This failing, for a failing assuredly it is, has its origin in human nature, and even in the best dispositions of human nature. Candour forgets the bad, but piously remembers the good, of what is gone. The failings of the dead are buried with them, while their virtues, whatever was attractive and engaging, are rescued from the grave; and acquire new splendour by being separated from every thing that offends. But, while offence lives, it arrests our principal attention; it irritates our tempers; it crosses our pursuits; it provokes our moral indignation; and therefore the vice of the day is the subject of everlasting complaint. But in all this proceeding neither truth nor justice is observed, and without ill intentions we often defeat every good purpose. The good and the bad of every day ought to be fairly stated, and neither candour nor prejudice ought to be heard at the bar of impartial justice. By exaggerated praise or exaggerated blame we give a sanction to folly, to error and vice, while we throw discouragement on the ingenuous pursuit of wisdom, truth and virtue.

In Rousseau we find a striking example of intemperate censure and intemperate praise. The preference of the past to the present, of barbarity to refinement, of ignorance to knowledge, is his favourite theme: it directly or obliquely insinuates itself into all his writings. Barbarity with him is simplicity; it is nature in her pure ingenuous walk; while refinement, taste and elegance are only the gilding of duplicity, fraud and vice. To know more than simple nature obtrudes upon us, is only to know the instruments of mischief; it is to awake a temptation, and capacitate man to be the enemy of himself and his fellow man. With a splenetic turn of mind he finds more to offend than please in the whole view around him; with a passion for fame he courts reputation by the singularity of his doctrine, by the boldest contradiction to the common sense of mankind; and with abilities wonderfully fitted to give a grace and a charm even to the grossest absurdities, he ventures on an open hostility to every thing which man conceives to be his highest ornament and praise.

I have observed that this humour of depreciating the present state of man, and overrating that of the past, tinctures most of the writings of Rousseau; but it is the professed object of the celebrated essay to which the academy of Dijon adjudged the prize. He maintains that the progress of society and its boasted improvements have been only to make man progressively acquainted with misery. He charges this crime particularly on the sciences and the arts. "Antient days, he says, were more virtuous than our own, and the degeneracy of our own days owes its origin to our knowledge." In fine, knowledge or science appears to him to be Pandora's box, replete with every evil. A poor prisoner in a house of lunatics, being asked the cause of his confinement, replied, that he thought the whole world to be mad, while the world thought him to be mad, but unhappily the world out-voted him. Roussseau must have thought, if he thought as he wrote, that a mania had possessed man from a very early period, and that in his day this mania had risen to its greatest height. It was well that the question did not come to issue between him and the world, for the world would assuredly have out-voted him. Rousseau is however entitled to a philosophic and argumentative reply.

In the first part of that discourse Rousseau objects to knowledge, that it is the parent of that external civility and politeness, by which the foundation of candor and plain dealing are undermined, and fellow-intercourse becomes constrained and disguised. Before art, says he, had fashioned our manners, imposed concealment on our passions, and taught us to speak a borrowed language, our behaviour though rustic, was natural. He admits that human nature, at the bottom, might not perhaps be better; but he asserts, that men derived security from being able to read each other's thoughts, and that this advantage, of which we now know not the value, preserved them from many vices. To this part of his charge the present essay is confined.

In answer to this charge it is asserted, that external civility and politeness are not the offspring of learning or knowledge, but claim other parents; that disguise, borrowed looks and language, and false exhibitions of the heart are not peculiar to any period or state of man; that sincerity and honesty are not irreconcileable with politeness; and that whatever of evil can be charged to the account of politeness, is amply compensated by the real good which it produces.

Politeness may certainly associate with learning, and may be separate from it; but ther first origin is in the good-will and sympathy of man, in the desire of being agreeable in the form as well as in the substance of our fellow-intercourse.

This is so obvious, that it is impossible to discover any special connection of cause and effect between a learned mind, and a polite mind. A learned man, without a kind and sympathetic heart, without a desire to please, may be as blunt a rustic as Rousseau can contemplate in his golden age of simplicity. Learning is very far from being the character of the polite world, and politeness in a still less degree is the character of the learned world. The weakest persons, to whom literature has not opened her very door, may lead in the dance of fashionable politeness. They are perfectly innocent, poor creatures! of the horrid crime of learning; but they are the arraigned before Rousseau's tribunal, they are the convicts of unmeaning profession, of prostituted language, and of all the idle waste of words. Observe the learned man! He may possibly be polite; he may be courteous in his address, in his speech, in all his manners; but he has not learnt this from his books; he has acquired it from an habitual commerce with the dressed and fashionable world. Such a union of attainments is however a rare spectacle; for, learning abstracted from other circumstances has a contrary tendency, and the world is so persuaded of this, that it expresses something like astonishment, if in the acknowledged scholar or philosopher it find the polite man. The love of retirement and even of solitude, as conducive to the pursuits of learned men; the little attraction which they feel for the lighter amusements of life, the straws in their estimation which float upon its surface; the little attention which they have bestowed in order to acquit themselves with propriety and grace; the disgust which is excited in them by the trifling conversation and important nothings of men of the world, render what is called good company as unfit for a philosopher as a philosopher is for good company. What a figure does he often exhibit in a gay and brilliant circle, with his solemn air, his stiffened attitudes, his unmanaged limbs, his absorbed mind, his inattentions, his constrained recollections, his studied expressions, his deep and sensatious discourse!

He is an object of ridicule to the circle around him; but he knows to estimate himself, and he returns the contempt with which he is received. He feels that he is not on his proper ground; no common sympathy attaches him to his company, nor his company to him; each are under restraint, but a modesty yet unsubdued in him subjects him to truly painful feelings, while a happy confidence which the polish of the world often confers, administers to the company the enjoyment of a secret triumph. He retires from the scene without regret, and his absence excites no regret in those whom he has quitted. A few reflections on the strange interview for a while occupy the thoughts of either party. The one laments the littlenesses and follies of which he has been a witness; the others laugh at the awkward mortal for his oddities and unaccommodating wisdom. While the fruit of these reflections differs in each as much as the reflections themselves. The one are strengthened in the persuasion that the accomplishments of politeness are the finishing of the human character, and with more self satisfaction go on in a course, which as a whole is but a waste of time, of talents and of character. The other owes it perhaps to his keen disgust that he is not swallowed up in the gulph of dissipation, that trifling and unimportant attentions are not over-rated by him, do not debauch his mind, nor lead him to the borders, if not into the open field of vice. There is certainly an extreme in the judgment of each, but with which extreme the just estimate of character and dignified enjoyment is most likely to be found, is left to the Genevan philosopher to calculate.

In fine, learning and politeness may be cotemporaneous events in the history of man, and they may each be derived from their proper source; but they are neither of them the parent of the other, nor do they kindly mix in any relationship or affinity. To derive politeness from learning, and charge to her account the vices of the former, whatever these vices be, merely because learning and politeness may be co-existent, is one of those sophims, which logicians class under the name of *non causa pro câusâ.* If the error proceed from design, it is criminal; if it proceed from ignorance, and a confused understanding, it is contemptible in one, who pretends to the character of a philosopher, and a censor of the follies of his day.

Learning and civility appear both to be derived from the same circumstances in the condition of man; but if civility be a vice, learning is not to be accused on this account. The field produces the grain and the weed. Is the salutary plant accused, because the useless or even the noxious plant vegetates by its side? As man betters his external condition, multiplies the conveniences of life, his views enlarge, he connects himself more closely with fellow man, he widens his connections and dependencies, and to accomplish this his intellect and ingenuity are provoked; his manners also become more insinuating; he courts his fellow by courtesy as well as by kindness; and thus science, the arts, urbanity and the agreeable in form, address and intercourse, advance with a step proportionate to the growing interests, utilities and conveniences of life.

This appears to me to be the true origin of learning and civility, considered in a general view, and to account for their co-temporaneous existence in every instance, whenever they have been found to enter into the human character. But learning and civility, though issuing from the same source, and always in some degree accompanying each other in their progress, may vary in their respective character from other circumstances; and therefore it may require the aid of some other cause to account for the peculiar politeness of later Europe, which is the very object of Rousseau's invective. To discriminate the urbanity of Greece and Rome from the politeness of the present century, and to account for whatever is peculiar to and characteristic of the latter, may not therefore be impertinent to the object of the present essay; especially if it shall appear that learning is as innocent of the guilt, which may be imputed to modern, as to antient manners.

Accurately to define the characters of antient urbanity and modern politeness, may be no easy task, nor is this difficulty peculiar to the present subject, as there are many instances, in which we cannot define, what we strongly feel. Cicero has given a definition of urbanity, which may apply to the civility of any day, as it is a mere definition of the abstract principle. Had he illustrated it with a familiar instance in the manner of his day, we should have been better able to estimate the standard of Roman urbanity. We have not a history of the private life of the Romans, but from some incidental facts which are recorded, we may decide that their manners were not so dressed and elegant and delicately finished as those of modern Europe. Yet we must allow to Greece and Rome all that polish which answers to their high condition of prosperity and cultivation. The wants of nature are common to all, but a richer possession of the external conveniences and accommodations of life cannot fail to awaken an artificial taste, the idea of a grace which may be superadded to every enjoyment. All of the polite and elegant which the collision of man with man can be expected to accomplish, may be ascribed to the higher personages of these antient days, but the influence of woman on manners could not be experienced by them, because woman held not the same rank in society as in these later times; and therefore this must constitute one characteristic feature in which the politeness of the most accomplished periods of Greece and Rome could bear no resemblance to that of modern days. Commerce, wealth, science, arts, and urbanity have made a rapid progress in Western Europe, and independant of any other cause must have produced their effects; but the state of woman, which is absolutely novel in the history of man, has given an appropriate character to our manners; it preceded every other cause; it operated in our rudest and most ignorant periods; it was the originating source, it has been the cradle, it is the highest aspiring of our politeness, and from this is derived whatever is peculiar in the manners of the masculine sex.

It is certain that woman had little estimation in antient times, unless as subservient to a purpose which it becomes me not to name before this assembly; or, as necessary to posterity; or, as the superior oeconomists of domestic supply and order. They were passed as a property from the parent to the husband; they were not introduced on the public stage of life; they were even secreted from the open face of day; they were not admitted to the councils of man, to an equal participation of the hopes and fears, the joys and sorrows of the lordly male. This, whether it were to the honour or reproach, whether it were the blessing or misfortune, of antient days, was assuredly the character of antient nations, though with some difference of degree; it was that of the Asiatics, of the Greeks and Romans, and continues at this day to be the unvaried character of the eastern world. To the rough inhabitants of northern Europe, barbarians as history affects to call them, but who were our progenitors, the female sex are primarily indebted for their vindication to the equal dignity and privileges of human nature. To this singular character of these northern nations, long unknown to the rest of the world, Cæsar and Tacitus have born testimony, while they were confined to their native land. The romantic gallantry of the days of chivalry, and the romanzas of the succeeding period, in which it is difficult to decide whether superstition, heroic courage, or idolatry of the sex, be the predominant feature, and the gradual passing of the old romance into the modern novel, all demonstrate that this character has never been parted with, but been mellowed into the more rational and tempered gallantry of the present day, such as we their posterity, the inhabitants of modern Europe, unreluctantly bow to. If man have imposed chains upon himself, he feels not the weight of them: he is happier in the participation of power and influence with the female, than when he held her under his absolute dominion, when she was the slave of his will, or the passive instrument of his selfish pleasures.

This generous sympathy of our northern progenitors with the partners whom nature and God designed for them, happily co-incided with the equal and liberal spirit of Christianity; and to these two powerful agents, which were nearly cotemporary, we owe that wonderful revolution of social and moral sentiment, which constitutes the distinction of later Europe. Woman has now been permitted to resume her proper rank in society, and to her we are greatly indebted for the present polish of ruder man; for that ease, propriety, grace, attention and desire to please in the manner of every intercourse, which offends the cynic eye of Rousseau. To every thing that is human some accusation may be brought, whether in consequence of defect, excess, or association, and therefore it is not wonderful if politeness committed to the management of men should be subjected to censure; but be her errors and excesses what they will,

Look on her face, and you'll forgive them all!

To her this generous acknowledgment is due, that she smooths the asperities of life, veils the deformities of selfish vice, and gives to ingenuous virtue and goodness its highest lustre.

I do not mean however in this part of my essay to be the advocate of politeness, but to prove, that learning is perfectly innocent of its birth, in modern as well as antient times, and let its present character be what it will, to fix the filiation upon its proper parents. If it be true, that the politeness of modern Europe derives itself from the restoration of woman to her proper rank in society, and that woman is the legislator of politeness; there is a circumstance in the character of woman, which demonstratively proves the folly of Rousseau in ascribing to learning either the blessings or the curses of politeness. In no day, not even in the present day, has woman been eminently distinguished for learning; and therefore the empire of politeness, sovereign as Rousseau supposes it to be, neither derives itself from, nor is maintained by learning.

I return to the direct subject of the essay. Having rescued learning from the imputation of politeness; I wish also to rescue politeness from the more odious charge of insincerity and dissimulation, as if they were her necessary and appropriate character. Dissimulation and artifice may and often do assume the attractive form of politeness, but they are not her natural offspring; they have their origin in those interested passions, which are common to man in every age and nation, and which pure and spotless innocence, if it have ever existed on this earth, is alone exempt from. The savage, the rustic and illiterate know to decieve under a fair exterior, whenever a selfish end awakes the desire, and deceive as artfully and as successfully as any of the polished and lettered sons of modern Europe. Instructed by mere interest, they can adopt even a seducive address, and in the very form of their natural simplicity and bluntness they can still more successfully deceive.

Uncultivated society has its virtues, it has its vices also, of which none hold a more distinguished rank than art, deceit, deliberate fraud and imposture. The history of the Arabians, Tartars, North-Americans and Chinese, who are at best only managed and disciplined savages, is much more the history of lying and imposture than of plain truth and honesty. They can practise insinuating address, they can mislead with deep designs, and deceive with fatal success. In their national treaties they can shroud their intentions with as artful concealment as the most lessoned adept of our diplomatic schools. With war in their hearts, they can send an embassador of peace, out of whose breast not the shrewdest politician of Europe can dig the secret. In war, they are treacherous more than brave; stratagem and ambuscade constitute their military tactics, and they measure their triumphs, not by the manly resistance which as men they have overcome, but by the insidious violation of faith, by springing upon their destined prey in all the confidence of security, by their merciless use of an inglorious victory, and the undistinguishing massacre of age and sex.

In this representation I may be supposed to exhibit the portrait only of the North-American tribes; but the history of every rude and uncivilized nation presents the same features; and in this very century the Chinese, whom I have not excepted from the class of barbarians, have exhibited a dreadful specimen of this insidious, unmanly, and savage character. The late emperor of China, jealous of a numerous and powerful Tartar horde, and, if I remember aright, one of the very tribe from which himself was descended, allured them by faithless promises, till he had drawn around them a cordon of his numerous hosts. Then in the true spirit of a savage, he issued the word, and appeased his fears by putting them all to the sword. He exterminated nearly three millions of human beings at a blow.

Such are the representatives of the native simplicity and innocence of undressed man; such are the patterns of frankness, sincerity and manliness, to whom Rousseau offers the incence of an almost ideot praise, in comparison to whom the European character is one uniform blot. He rivals the Papal power in its greatest plenitude; he blackens an angel, and whitens a devil.

But if contrary to fact, and what the history of man in the progress of society reports to the plain enquirer after truth, it should be allowed that politeness derives itself immediately from learning, yet learning is not therefore to be charged with that duplicity and dissimulation, which in the practice of politeness has excited the indignation of Rousseau. Politeness in its primary designation presents a fair and amiable character, but like every blessing to man, vice may seize the blessing, and as far as vice can operate, may convert the blessing to a curse. Has learning taught to vice this lesson? If this be a truth, it presents a novel idea of learning, contrary to every idea which has been entertained of her genius and spirit; for before the Genevan philosopher was pleased to undeceive man, she was supposed to be favourable to truth, to the discovery and to the ingenuous communication of truth.

What gave to the openings of science that charm, which roused man from the indolence and torpor of savage life, but that it ministered to this native love of truth in the mind of man, to that ardour of a rational mind, which, when awakened, pursues with avidity and with unsated gratification the discoveries which nature unfolds to the diligent enquirer. This is the spirit and genius of learning, and most unlikely to have inspired the wily arts of polished life.

The question is not therefore, whether politeness, and even learning, may not be seized by vicious men, and perverted to vicious purposes, but whether it be of their genius and character to favour this abuse. The question is, whether politeness be not of a virtuous origin; whether it be not this very circumstance which recommends her to vice; whether science, whose object is truth, and the communication of truth, can intend disguise, deceit and perfidy. Vice exists. Granted. Let the crimination be carried to the baser passions, which originate vice of whatever form, but not to those laudable passions of man, which contemplate truth and kindness.

How many other blessings does vice pervert and abuse? From the Creator of man is derived the very capacity of learning, the impulse to, the delight therein, and all that is the genuine offspring of learning. If learning is to be accused, because vice may prostitute learning itself, and whatever is the progeny of learning, to uses which the generous spirit of learning abhors; then *a ſortiori*, the Creator is to be accused, who has inspired in man the desire and the capacity of learning; nor is there one gift, nor one blessing for which we may not with equal justice reproach our maker, as there is not one gift nor one blessing which vice may not, and unfortunately does not, equally misapply. It is not of the genius of learning and wisdom to favour vice in any of its forms; science and virtue are naturally allied, and to the sons of learning the world is indebted for the most animated and persuasive pleas in the cause of virtue; that the wisdom of her ways has been illustrated; and that vice has been proved to be as foolish, as it is detestable. To learning this is an easy, a pleasant and a natural office; while all attempts to press her into the service of vice, have reflected disgrace and infamy on the perpetrators of so rude a violation. However individual men may pervert the plan of their Maker, science and virtue are united in the service of man, as wisdom and holiness in the conduct of the divine being. I make no apology for this momentary use of the stile of the theologist; it becomes me as a philosopher, who demands for theology a place in the great philosophy of nature. He who designed man, designed science and wisdom to be of the highest attributes of his being, and to traduce them, is to traduce both man and God.

But to the innocence of learning as not answerable for the duplicity and fraud and dishonesty, which Rousseau charges upon politeness, it may be farther added to its praise, that if any thing can guard a man against the impositions of dissembling address and language, it is to the cultivation of the intellect that he must be specially indebted for this guard. Learning places a man upon a higher ground; it confers upon him the advantages of superior intelligence; and when directed to the judgment of character, and assisted by an acquaintance with men, their objects, their modes, and varied import of language, it acquires a kind of intention into the very heart. A scientific man is so accustomed to yield conviction only to evidence, that he is rescued from that precipitate decision which is the pit of fools; he is possessed of more nice discernment, more accuracy in weighing every thing in the scale of sober judgment, more facility in resolving, combining, comparing, deciding; so that if imposture must haunt the best walks of life, no man can pass these walks with more security than the well-educated and well-informed man. I do not ascribe this praise to the verbal critic, the mere mathematician, or the simple sciolist of any form; but to him, who has studied man as well as books, which alone deserves the name of true science. And it is to this science that Rousseau's accusation can alone apply; for the learned only, who mix with men, can give to imposture those advantages, which he supposes learning to furnish. It is but in a small degree that learning and politeness can aid the dishonest and the knave; but it is in a very high degree that learning and a polished acquaintance with men can protect against the knave. I might illustrate the truth of this observation by an appeal to very striking facts, facts of the greatest magnitude and importance. Since the study of literature and the study of man have united to enlighten the mind, to give to it more intuition and vigour, false taste, false philosophy, false religion, and, I may add, false politics, have lost their power of fascination. True science, like Ithuriel's spear, when applied to satan in the guise of an angel, has compelled them to unveil, and stand forth to view in their naked deformity.

Having rescued learning from the imputation of politeness, it is an act of justice to rescue politeness from the imputation of insincerity and dishonesty, which Rousseau supposes to be her necessary attendants.

Every virtue and excellence of man invites a counterfeit, which assumes an honoured name, and tricks herself out in a respected garb and form, but has nothing within of the loveliness and worth of the original. It is thus that for a while, and for ever with the ignorant and the credulous, supersition may be mistaken for religion, the traitor for the patriot, the sycophant for the friend, the seducer for the lover, and the hypocrite for the honest man. But the fraud is discovered, and while the imposture sinks into irredeemable contempt and infamy, the solid virtue remains unimpeached, and acquires renewed splendour from the odious contrast. Thus it is with politeness; her origin is honourable; her foundation is firm; her seat is in the heart; her range of action is the whole intercourse of man; but her favoured residence is the breast of woman; woman, who with

> Love in her eye, and grace in all her walk,

was designed to allure, to attract, to scatter roses on all the paths of life.

Such as is woman in her best character, such is politeness. She visits undressed uncultivated man; in her whole intention, in her whole manner she is kind. The ruggedness of manners is smoothed under her steps, her smiles calm the turbulence of passion, and man is not allowed to approach man but in the form and language of kindness. The ingenuously honest and good embrace her in sincerity; the designing and bad have no access but in her name, and their hypocrisy is so far from being to her dishonour, that it is the homage which even vice finds itself obliged to render to her excellence and worth.—But to quit the poetic stile, which is not natural to me, the truth assuredly is, that politeness has not in her contemplation to furnish a language for the insincere and dishonest; her intention is in the less important and ordinary intercourses of human beings, which singly seem of small worth, but altogether make up the sum and mass of social enjoyment, to intermix those little sweet courtesies, which give a grace, a charm, an acceptableness to every thing. She is therefore of the family of good-will and benevolence, and he who cannot separate her genuine character and spirit, from the abuses which have attached themselves to her, deserves not the name of a philosopher, and hardly the name of a man. These abuses however, like most incidental evils, have provided their own correction. The ordinary forms and language of politeness have their appreciated value; they pass for no more than they are worth; while the higher value of the heart, of which politeness was designed to be the agreeable expression, is estimated by more accurate and certain tests.

I may add to what I have already observed in opposition to the magnified evil of a polished language, that there is something in ingenuous truth and honesty, which cannot be counterfeited, and which no artful language can convey. In a greater degree than is imagined does the mouth speak from the heart, and therefore along with what is common to truth and to imposture, to simplicity and to art, there is something which discriminates the one from the other; there is a reserve, a modesty, a delicacy in the one; a prurience, a boldness and a grossness in the other, which constitute a marked distinction of character, and tell the truth to a discerning world. Sure there must be an angel of detection, which follows the dissembler in his path, and compels the dishonest and designing to reveal the precious secret which they would hide. It is certainly the interest of the impostor to appear to be the perfect character which he assumes, but not all his practice in the art, not all the cool deliberation of his plan, not all his knowledge of the world, aided by genius and by learning, can enable him perfectly to act the part, which it is his interest to personate. Truth cannot betray itself, for having nothing to conceal, it has nothing to betray; but Art has, and therefore cannot guard every avenue of detection; in the very texture of its art it cannot present any thing like the simple and beautiful work of nature. Innocence and guilt are essentially different in their nature, and so in a great degree is their appearance, in spite of a general conformity of exterior. The artful courtezan cannot personate the inimitable simplicity, ease and grace of the untainted female.

In fine, vice must be allowed to exist in the polished and the learned world, and perhaps in more multiplied forms than in ruder and ignorant periods, though with less deformity and horror. But conceding this, it is still a truth, that whatever be the moral reproach of any day, learning and politeness are not the cause, and they are still a real blessing to the nation which possesses them. I may sum up their praise in a few words; they add to the acceptableness of real worth and goodness, they lessen the deformity of vice, and on the whole contribute largely to the melioration of society, to the general stock of human comfort and happiness.

ESSAY II.

That luxury and corrupt manners are not the progeny of science and the arts, in answer to Rousseau.

In the same celebrated essay to which the Academy of Dijon adjudged the prize, Rousseau aggravates the charge against science and the arts, by imputing to them the introduction of luxury and a general corruption of manners. In this I do not scruple to charge upon him the same confused and illogical understanding as in the former accusation. If he find but a cotemporaneous existence of two facts, he hastily concludes that the one is the cause of the other. Now as an argument is deemed to be bad, which concludes too much, so a rule of assigning a special cause to an effect ought to be considered as bad, which infers too much. He might, if he had pleased, and he ought, whether he had pleased or no, to have observed a multitude of other characters to be co-existent with luxury, such as law, political science, philosophy natural and moral, christianity, deism, atheism; and for aught that I can observe of a connection of cause and effect in what he has assigned, he might by the same rule and with equal propriety, have referred to any one or to all of these conjointly, the introduction of luxury and that corruption of manners, which is opposed to simplicity.—Sir Isaac Newton has in physics established this rule of judging of cause and effect, that, where two phenomena are invariably found to be co-existent, and where the disappearance of the one is instantly attended with the disappearance of the other, and the re-introduction of the one withdrawn is immediately followed with the re-appearance of the other, and so uniformly in every instance; we may then safely conclude that there is a connection of cause and effect between these phenomena. But this rule of judging is only meant to be applied, where the *actus operandi* and the *modus operandi* are concealed from our view. Now no one can pretend, that if learning generate luxury, the very act, and the whole operation, can be concealed from our observation as in the history of natural causes. It is incumbent therefore on any one, who criminates learning as the operating cause of luxury, to state in clear terms and agreeably to obvious experience, the will, the act, and the history in the production of luxury by learning. This Rousseau has failed to do, and this every one will fail to do, who shall be hardy enough to make the attempt.

But admitting that the rule of Sir Isaac Newton is equally applicable to the discovery of moral as of natural causes, which perhaps it is, though the application be unnecessary; yet Rousseau would derive no advantage from the concession, for his conclusion would fail if examined by the test to which the guarded mind of this sagacious philosopher has subjected his rule. The phenomena of learning and luxury are not always united, nor, having been previously united, is the disappearance of learning always followed by the disappearance of luxury. Let the court of Caligula, of Nero, of Heliogabulus, the Asiatic Monarchies, and many other striking examples in antient and modern history make the reply? The truth is, there is nothing in the character of learning, which inclines her to luxury, but there is much that is unfavourable and averse to it. The Antonines were learned princes, particularly Aurelius, and they were equally eminent for temperance and moderation, for sober and chaste manners. A learned man may indeed be luxurious, but instances of this mixed character are singular; for learning does not smile upon luxury, nor is luxury propitious to learning.

Luxury is a general term, and answers to very different standards in different minds, and in different circumstances. What a cynic would call luxury, a more correct judge of manners would denominate taste and elegance. But it is to the praise of learning that taste and elegance are of her progeny; if not, it is a vice in the most learned of all beings, that he has embellished his creation with such a profusion of beauty and elegance, and exhibited to us those perpetual models, which according to Rousseau have misled man.—The Genevan philosopher finds luxury and corrupt manners in learned nations alone. But with a more discerning eye, and a less prejudiced mind, he would have found them with equal excess, and with equal or more deformity, in rude and illiterate nations. The feast of a Kamschadale or an Esquimaux on a whale, a bear or an otter, is a true and proper luxury, and marked with as intemperate indulgence, as the rich and varied board of Lucullus or Apicius. The mind is of the same character in both; they differ only in external circumstances, and condition being changed, but the mind unaltered, Apicius would have been the Esquimaux. The corrupt manners of the Otaheitans have not been surpassed, nor perhaps equalled by any of the learned nations, to whom the unfriendly eye of Rousseau is directed. Minds more surrendered to effeminate luxurious ease and every lust have not been exhibited in the page of history; and yet these are an unlettered people, and strangers to the arts, which have vitiated modern Europe. They know not this wicked thing, called learning, which has brought in luxury and all prostitute manners like a flood upon us; nor those arts, which have been the panders of this luxury and corruption.

Rousseau seems to feel the difficulty of fixing immediately upon learning the crime of luxury and its corrupt attendants; but he is more at home when he refers them directly to the arts, which he supposes to be the offspring of learning. Now in this statement, and in the first hasty view of it, there does appear to be a connection between luxury and the arts, and between the arts and learning. But the first view of a subject is not always the true one, and there will be found in this, as in the former instance, the same inaccuracy, the same confused understanding, the same unfounded presumptions, the same illegal conclusion.

That luxury and very corrupt manners may exist without the arts has already appeared from what has been noticed in some of the rudest and most uncivilized nations, to whom may be added the Turks and the Moguls, examples of notoriety and magnitude, who from the commencement of their empires to the present day have been singularly hostile to science and the arts, but not hostile to luxury and the corrupt manners, which are justly supposed to be in her train. A nation ignorant of and unexperienced in the arts will and must, in proportion as it is possessed of the means, be luxurious and corrupt. Indolent ease finds the mind vacant to luxury and lust; this is in the constitution of human nature, and is verified in the constant history of national and individual man. A field of active exertion is therefore provided for man as his refuge; it gives a better direction to his mind; it arouses him from that listless repose, in which he has nothing to brood upon but the indulgence of his appetites and lusts, to which in such a state he is so prone, and which in such a state are almot necessary to support the burthen of existence. Hunting and war alone can rouse the savage to action, and then he is more temperate and abstemious than the philosopher or the hermit. The inexhausted allurements of scientific pursuit or the activity of the arts provide a more constant and salutary refuge to a civilized and wealthy nation from their devouring passions and lusts.

Science and the arts are therefore more properly the moral friend and guardian of man; they may minister to some of the productions which a luxurious and corrupt mind seizes upon, but they are innocent of the misuse, they neither suggest nor favour the misuse. The earth itself produces a very considerable part of the food of luxury. Is the earth to be accused as the criminal minister of luxury? The earth and science and the arts are all liberal in their gifts to man; these gifts are in their nature directed to utility and to blessing; they are in the reach of the virtuous and the vicious; virtue enjoys them in their proper character; vice converts them into a curse. To maintain the theory of Rousseau, the earth and science and the arts should be more sparing in their gifts, and all the virtue which his theory aspires to is the impossibility of being vicious. The earth and the arts resemble each other in the variety and richness of their produce, and in not directing nor answering for the use to which their productions shall be applied. But to science are annexed mind and will, and to the deliberate mind and will of science cannot be charged one approbation of abuse, excess, intemperance, luxury, or licentious, corrupt and profligate manners.

There is therefore no natural, no necessary connection between the arts and luxury or vice. The productions of the arts are illustrious monuments of human ingenuity, and in no small degree imitative of the superior productions of the great Artist; they are in their nature innocent of all criminal construction and tendency; they are much more applicable to virtue and to happiness than to vice and misery; luxury and corruption may exist without them, and therefore it is to other parents and to other causes that we must refer their existence and progress.

If luxury do not derive herself from the arts, however the arts may be violently forced into her service, learning is acquitted of the charge, though the arts should be allowed to be the progeny of learning. For learning, no more than God, is to be interdicted and accused in her most honourable walk, of contributing to the ornament, utility and happiness of man, because vice may seize upon and misuse her gifts. But the argument of Rousseau is even more unfounded in the supposed connection between learning and the arts, for the arts do not perhaps at all derive themselves from learning as their source, and in their progress derive a very inconsiderable part of their utmost improvement therefrom.

The elements of the arts, and many of the most valuable discoveries on which the practice of the arts depends, are derived from unlearned men, or are traced up to unlearned and, what at this day we call, barbarous periods. Who can name the learned day, in which the lever, the moveable pully, the wheel, the inclined plane, and the screw have been introduced to the knowledge and to the use of men? The arch, the pillar, the roof, and much perhaps of the stability, proportion and ornament of architecture, are probably indebted for their first conception to unlearned and unscientific men. The Greeks do not appear to have considered the Persians as a lettered and scientific nation, and though the vanity of the Greeks may render them very justly suspected in their estimate of foreign merit, yet it is probable that in this instance they erred not much from the truth. But the ruins of Persepolis present the idea of a structure, which might have rivalled the proudest monument of Grecian architecture. They are at this day the admiration of European artists, to whose judgment and taste the highest defference is paid. To rude ancestors we owe the first idea of a ship, and no inconsiderable progress to that complex and wonderful state in which it now exists. To the Greeks and Romans in their more rude and unlearned state we are indebted, if a debt it may be deemed, for the discipline, the order, the combinations, the evolutions and the general tactics of war; nor, unless perhaps in the application of gunpowder has all the science and ingenuity of the moderns much surpassed them in this dangerous art. The practice of astronomy, though without a sufficient knowledge of its theory, yet founded on principles derived from an observance of the motion of the planetary bodies, is of very remote antiquity, and has been applied with considerable accuracy by nations of no scientific character, and but in a moderate degree removed from barbarism. If a simple elementary language, wherein from a few characters infinite combinations are formed, be a necessary instrument to the progress of science, the Chinese can have no pretensions to the character of a learned nation, although for no other reasons this attribute should be refused to them. For as language is the vehicle of ideas, how slow must be the progress of literary improvement, where only to know the language itself requires the application of a whole life. Yet on the first visit of the Europeans the Chinese were found to be possessed of the elements of almost all the arts, those very elements, which under the culture of the more ardent European have so exalted him amongst men. The same may be observed of the Mexicans and Peruvians, who had no form of written language whatever. The date of their empires was indeed comparatively of yesterday, but at the period of their highest improvement they could support no claim to the character of learned nations. Yet many of their productions of art, magnificence and taste were objects of admiration to the more improved European. The practical principles of chemistry have been known, and successively acted on, by many of the rudest and most ignorant nations of the earth, and the communication of some of their processes would be a valuable acquisition to the European artist of the present day. Indeed without disparagement to the present state of scientific improvement it must be acknowledged that the valuable arts of mechanics and chemistry have been indebted for discoveries of high estimation to rude and unlettered practitioners, who had eluded all the penetration of the theoretic artist. The fact appears to be, that mere accidental observation, excited by the continually working hand of nature, and agreeably to those eternal laws which govern her operations, has revealed to man, in every state and condition, the fundamental principles of all the arts; that they are thus brought home to his very feeling, and that, the discovery being made, the necessities and interests of man seize the discovery, and apply it to his use. Man, in a state of literary culture, digests these experimental discoveries, compares them, reasons upon them, and reduces them into an orderly and harmonious system, which is without doubt of great assistance, in applying the practice of the arts to progressive improvement and utility. Sensible of this truth, the ingenious theorist will acknowledge that the arts, in the whole extent of their subservience to the use of man, have derived their richest treasures from the discoveries and operations of rude and unlearned men.

Rousseau therefore fails in every view in which it can be attempted to fix the odium of luxury and its concomitant vices upon learning. There is no neceſsary connection between the arts and luxury, and the arts, in what degree they may be required to minister to luxury, aſk little, very little, aid from learning.

Perhaps Rouſseau had no view to honeſt truth in this celebrated eſsay, but by a bold ſingularity to raiſe himſelf into general notice. Had truth been his object, he could not have avoided to obſerve, what muſt ſtrike the common mind, that the appetites and taſtes of men are the parents of luxury, and that wealth, or a ſupply of what wealth purchaſes, is the nurſe of luxury. Wherever or whenever theſe two are found to be co-exiſtent, luxury in a greater or leſs degree will be found to exiſt alſo. Theſe may be co-exiſtent, and to any extent, and have ſo exiſted, without any thing of what anſwers to the ſcientific arts of modern Europe.

OBSERVATIONS *on the* NERVOUS SYSTEMS *of* DIFFERENT ANIMALS; *on* ORIGINAL DEFECTS *in the* NERVOUS SYSTEM *of the* HUMAN SPECIES *and their* INFLUENCE *on* SENSATION *and* VOLUNTARY MOTION.

BY JOHN HULL, M.D.

READ NOVEMBER 28th, 1800.*

> " Le mécanisme des sensations, le rapport des nerfs avec le sujet senti à la surface du corps & avec le centre, où se réunit le sentiment, sont aussi obscurs, qu' ils l' ont toujours été, malgré tous les faits, qu' on a recueillis depuis plusieurs siecles." FOURCROY *Syst. des Conn. Chim.* T. IX. p. 348.

In another publication I have enumerated some of the most remarkable deviations from the natural form, that have been observed in human fetuses on the side of excess, both in the number and proportion of their parts. Astonishing as these may appear, the deviations on the side of defect are not less so and the latter are perhaps more particularly interesting to the anatomist and physiologist, as serving to point out the comparative necessity of the different parts, concerned in the performance of the various functions of the animal economy. Numerous instances might be adduced, wherein one, or more of the most important organs of the human body have been found wanting in the fetal state. But, passing over the defects, which occasionally take place in the organs of respiration, circulation, digestion, generation, &c, I shall in this paper confine myself to the consideration of the defects, observable in the nervous system of man, and their influence on the important faculties of sensation and voluntary motion; premising some general observations on the diversity of parts, structure, substance, texture, proportion, laws, &c, of the nervous system of man and the inferior animals in their natural, or perfect state,

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* Some additions have been made to this paper, since it was read to the society.

§ I.

On the Nervous System in the different classes of Animals.

By a celebrated modern writer on Comparative Anatomy, M. Cuvier, animals are referred to two grand divisions: The *first* comprehending all those, which have a dorsal spine, or vertebræ: The *second* comprehending all those, which are destitute of vertebræ.

To the former division are referred the first four Linnean classes of animals, namely, *Mammalia*, *Aves*, *Amphibia*, and *Pisces*. To the latter belong the two remaining classes of *Insecta* and

Vermes. As there is a general resemblance in the nervous systems of the animals, referred to each of these great divisions, particularly the former, I find it convenient to adopt them here.

1. The nervous system of man and other animals, included in the first division, is more extensive and complicated than in insects and worms. Viewed with respect to its situation, it consists of three primary parts, namely, the *Encephalon, Spinal Marrow* and *Nerves.* The two former have been considered as constituting the common trunk, the last as the branches of the system.

The ENCEPHALON, which is so named from being seated in the head, is subdivided by anatomists into three principal parts, the *cerebrum*, or *brain* strictly so called, the *cerebellum* and *medulla oblongata*: But for brevity and to avoid the unnecessary use of technical terms I shall, in general, speak of these three parts collectively under the title of *brain*, taking care to guard against ambiguity, when it becomes necessary to mention any of the three parts distinctly from the others. It may not be improper to state here, that in those animals, which have no skull, and therefore, strictly speaking, no encephalon, that portion of the nervous system, which is the most bulky and occupies the highest, or foremost place, will be named brain, since this term is adopted by writers on comparative anatomy.

The SPINAL MARROW is so named from its occupying the canal, formed by the vertebræ, or bones of the spine, and is generally considered as a continuation, or production of the brain.*—In animals, which have no vertebræ, that part of the nervous system, which is continued from the brain and is connected with the nerves, is also named spinal marrow, though not very properly.

The NERVES are cordlike bodies, rising in pairs from the brain and spinal marrow and ramifying in order to be distributed to the different parts, that are influenced by them.—That extremity of a nerve, which is attached to the brain, or spinal marrow, is usually named its origin[*]; whilst that, which is distributed to the organs of sense, muscles &c, &c, is named simply its termination and by some writers its sentient extremity, when so disposed as to receive impressions from external bodies. Of the ganglions and plexuses of nerves it is not necessary to take notice in this place.

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* By some writers the spinal marrow is considered as the largest nerve of the body, but improperly according to Soemmerring: "Notæ enim ejus ab omnium nervorum notis sunt diversæ, cerebri vero notis respondent. Etenim 1. Non ea est medullæ spinæ structura filata, quæ omnibus quidem nervis. 2. Mollior est nervo. 3. Intrinsecus, ut aliæ quædam cerebri partes, portionem cineream continet. 4. Eodem modo ex ea nervi oriuntur, ut v. g. ex cerebro tertius & sextus nervus, nequaquam vero ea ratione, qua nervi ex truncis, qui dividuntur, vel ex gangliis oriuntur. 5. Bestiis maxima pars est massæ cerebri. 6. Homini, ratione cerebri habita, minor est, quam ulli alii animali. 7. Stimuli metallorum per spinæ medullam spasmos non excitant, ut per nervos." *De Corp. Hum. Fab.* T. IV. p. 81.

The nervous system is composed of two principal *substances*,[†] named the cineritious, or cortical, and medullary; which differ considerably from each other in their colour, consistence, and other properties: The former being of a reddish ash colour, semitransparent, softer, without distinct fibres, and possessing little, if any, sensibility: The latter being white or yellowish, opake, firmer, consisting of very fine fibres variously disposed and evidently possessing sensibility. Both these substances are very evident in the brain and spinal marrow: But the cineritious matter is not so easily observed in the nerves, being in a very small proportion to the medullary. Dr. Monro, however, informs us, that he has by an attentive examination found it in most of the nerves.† The relative position of the cineritious and medullary substances differs in different parts of the nervous system. The *consistence* of the brain is softer in cold-blooded animals and in some fishes is almost fluid.

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[*] Dr. Monro thinks this extremity is improperly so named and assigns a good reason, which will be noticed hereafter.

[†] Besides these, there are two other substances noticed by anatomists, namely, 1. The *portio intermedia*, which is whitish or yellowish, and is found in the cineritious substance of the posterior lobes of the cerebrum. 2. The *portio nigra*, which is found in the crura of the cerebrum. These substances are only found in the brain and are inconsiderable. See Soemmerring, T. IV. p. 47.

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† "Although the nerves have been universally considered as a continuation of the pure medullary substance of the brain and cerebellum; yet I find, on accurately examining them, that with a few exceptions, particularly of the optic nerves and portio mollis of the auditory, they are all of a browner colour than the medullary substance, their pia mater seeming to furnish a quantity of cineritious matter."—"The optic nerves and portio mollis of the auditory seem, indeed from their bright white colour, to receive from their pia mater little or no cineritious matter in their progress to the eye and ear: but, as soon as they enter these organs to form the retina and to be spread out on the membrane of the cochlea and semicircular canals, instead of remaining white and opake, they become cineritious. The cause of which is, that, contrary to what has been alleged by all authors, they carry with them their pia mater, and from that membrane every fibre of the nerve receives cineritious matter." *Observations on the Structure, &c. of the Nervous System, p.* 32.

The *structure* of the nerves and spinal marrow is very simple, compared with that of the brain. The last possesses a complicated and beautiful organization, which we shall perhaps never understand. In viewing the structure of the heart, we easily comprehend, in what manner each of its parts contributes to the peculiar function of this organ, the circulation of the blood: But we do not perceive how any one of the cavities, processes, &c, &c, of the brain contributes to sensation, volition, or the intellectual functions. It will not therefore be necessary for my present purpose to enter particularly into the consideration of the structure of the brain in man and still less so in the other animals, referred to this division. It may be proper, however, just to remark, that in each of the higher classes of animals, the brain has its peculiar characters, arising from the presence, or absence of certain parts, or from the position &c of these, and that M. Cuvier has specified these peculiarities.* The spinal marrow and nerves are of a fibrous structure, and the fibres of the latter, except at their remote extremities, are included in both a proper and a common coat, or sheath.

The *proportion* of the brain, compared with the rest of the nervous system, varies in different animals. In the higher orders, the brain is larger in proportion to the spinal marrow, and the spinal marrow is also larger in proportion to the nerves connected with it. The human brain is by much the largest in proportion to the rest of the nervous system: It commonly weighs from two to three pounds, or upwards; but very rarely reaches four pounds. In other warm-blooded animals the brain diminishes in volume, in proportion as the spinal marrow enlarges, and in some fishes the bulk of the two other parts of the brain scarcely surpasses that of the medulla oblongata, and this scarcely exceeds the spinal marrow. The intellectual powers of animals seem to correspond in extent with the proportion of the brain to the rest of the nervous system, and the perfection of its organization: Whilst the acuteness of sensation and the force and rapidity of voluntary motion appear to depend upon the proportion of nerve, distributed to the organs of sense and voluntary motion rather than on the proportion of brain. All animals, which are as large as man, have larger nerves and greater strength. The smaller animals have much larger nerves in proportion to their limbs. Fishes, which have very small brains, have very acute feeling and move with great rapidity and force. Many animals have more acute senses and greater powers of motion than man; although all are infinitely inferior to him in intellectual powers.

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* Anatomie Comparée. T. II. p. 172—175.

All the animals, which have vertebræ, have the same number of senses as man.

2. The animals, which are destitute of vertebræ, when viewed with respect to their nervous systems, may be properly subdivided into three orders.

The *first* comprehending the insects and some worms; which have a brain, spinal marrow, and nerves, or three parts corresponding to these at least. The *brain* in these animals is placed above the alimentary canal and sends off two branches, or legs, which inclose the œsophagus like a collar.—The continuation of this, or the *spinal marrow*, is situated under the alimentary canal and contained in the same cavity with the other viscera. It is double, the two legs remaining distinct throughout a great part of their length and being only united, at different points, by means of the knots or protuberances, from whence the nerves arise, and which are nearly as large as the brain. Mr. Cuvier observes, that " the great sympathetic nerve, which is constantly found in all animals with red blood, does not exist in any white-blooded animal, unless we consider as such the two nervous threads, which unite all the ganglions and which are named *moelle épinière* in the crustaceous animals, insects and worms: In which case these animals would have no spinal marrow, and the absence of this medullary production would be the common character of all the animals with white blood."*

The *second* includes some *Mollusca*, which have a brain and nerves passing off from it in a radiated manner, but no elongation of the brain, analogous to the spinal marrow of the former order. There are however scattered ganglions, almost as large as the brain itself,† in various parts of the body.

The *third* comprehends those animals, which are of a gelatinous texture and have no evident nervous system as many Zoophytes, or Polypes. In these nothing corresponding to brain, spinal marrow, or nerves, can be discovered; nor can vessels or muscular fibres even be detected. ‡

Thus we find, as we descend in the scale of animal creation, the common trunk of the nervous system gradually lessening and the medullary substance less and less concentrated, till it becomes at length imperceptible and equally distributed amongst all the parts of the most simple animals.

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* Anatomie Comparée T. II. p. 124.

† Ibid. p. 98.

‡ " La faculté de sentir & celle de se contracter, qui dans la plupart des animaux sont exclusivement propres, l'une à la substance nerveuse & l'autre à la fibre charnue, paroissent être également répandues dans toutes les parties de certains animaux gélatineux, dans lesquels on n' apperçoit ni fibres ni nerfs." *Cuvier Anat. Comp. T. I. p. 27.*

The number of senses in this division is less than in the animals, that have vertebræ. Sight is wanting in some of them; hearing in a greater number: But the remaining senses, especially feeling, appear to be never wanting.† Have these animals any sense, which is not possessed by those of the former division? Some ingenious physiologists think this probable.

I shall in the next place mention a few of the *phænomena*, or, as they are usually termed, *laws* of the nervous system, relative to sensation and voluntary motion in different animals.

1. *a.* In man and other animals, belonging to the higher classes, when naturally formed, no sensation is excited by an impression, made upon any part of the body, if the nerves, distributed to that particular part, be divided, tied, or so strongly compressed in any part of their course as to destroy the communication betwixt it and the brain, or spinal marrow. Hence it is proved, that neither the termination of the nerve, nor any part of it below its connection with the brain, or spinal marrow, is the seat of sensation.

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† Cuvier Anat. Comp. T. I. p. 37.

b. Neither is sensation excited by an impression made upon a part, the nerves of which are connected with the spinal marrow, if the spinal marrow be divided, or strongly compressed, above the point, where these nerves are joined to it. Hence it is equally proved, that the spinal marrow below its junction with the brain is not the seat of sensation.

c. If the whole of the nerves of a limb, or any part, be divided, or so strongly compressed, as to intercept all communication betwixt the limb or part, and the brain, no motion can be excited in the former by volition: Consequently the power of beginning voluntary motion is not in the nerves.

d. If the spinal marrow be divided, or strongly compressed, in the neck or any lower point, none of the parts, supplied with nerves from the spinal marrow below the injured part, can be excited to action by the will. Hence it is proved, that the power of beginning voluntary motion is not resident in the spinal marrow.

e. When the spinal marrow is injured in the manner just mentioned, the mental faculties are not necessarily impaired, and sensation may be excited and voluntary motion produced in those parts, which are supplied with nerves from the brain, or from the spinal marrow, above the injured part. Hence it is demonstrated, that the brain, or the junction of this with the spinal marrow and nerves, is the exclusive seat of sensation, of the power of beginning voluntary motion and of the intellectual powers in man (and other animals), whose nervous system is naturally formed and not affected by any gradual disease. A further proof of this is derived from hence, that a sudden and violent compression of the brain destroys all sensation, voluntary motion and intellectual powers.

Upon this matter physiologists are generally agreed; but they differ very considerably as to the part* of the brain, which is the immediate

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* "Sedes animi auctore *Cartesio* est in hypophysi, *Bontekoe*, *Lancisio* & *La Peyronie* auctoribus in corpore calloso, *Digby* auctore in septo cerebri medio, *Vieusseno* in maximo medullæ orbe, *aliis* in colliculis nervorum opticorum, *aliis* in nodo cerebri, *Arantio* in ventriculo cerebri tertio, *Willisio* in corporibus striatis, *Drelincurtio* in cerebello, *Miegio* in spinæ medulla."

"Verum tamen hypophysis ab *Hallero*, *Virideto*, & a me sine ingenii labe læsa est inventa; corpus callosum *Heuermann*, *Zinn*, *Lorry* & *Laghi* sine animi labe læsum invenerunt, verum tamen hæc experimenta, super bestiis capta, parum certi super labe ingenii produnt, *la Peyronie* vero observationes in hominibus instituit. Septum cerebri medium non raro sine animi labe ex cerebri hydrope laborat—Corpora bigemina *Viridet* læsa invenit—Cerebellum plures viderunt vitiatum—Corpora striata *la Peyronie* læsa vidit—Spinæ medullam *ego* sæpius sine ingenii labe in ominibus læsum inveni." Soemmerring de Corp. Hum. Fabrica T. IV. p. 100.

Soemmerring seems inclined to consider the liquor of seat of sensation, volition and reason, which directly influences and is influenced by the immaterial part of man, and which is by many writers named the *Sensorium Commune, Seat of the Soul*, and lately by Fourcroy *Sensibilité Centrale*. There are few, if any parts of the encephalon, which have not upon some occasion been injured or diseased without any remarkable diminution of the vital and mental powers. If any one part can with propriety be fixed upon, as entitled to this distinction, it should be a part, which cannot be injured without affecting sensation, volition, &c, and which is found in all animals, that have a visible nervous system. The most constant part of the encephalon, according to M. Cuvier,* is the cerebellum. Haller thinks it not improbable, that the seat of the soul is in the beginning of every nerve and that the conjoined first origins of all the nerves constitute the true sensorium commune. — " Et denique per conjecturam non absurdam, ubi initium est nervi cujuscunque, ut omnium ner-

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the ventricles as the seat of the sensorium commune. He says " Peculiare organum sensorii communis si ponere fas sit, vel si propria sedes sensorio communi in cerebro est, haud sine veri quadam specie hoc in humore quæri debet." Ibid p. 69. See also his Dissertation on the Organ of the Soul. *Berol.* 1796. 4.

* Anat. Comparée Tome II. p. 109. & p. 121.

vorum junctæ primæ origines efficiant verum sensorium commune."*

The following case, whilst it, in common with many others, shews the excessive injury, nay almost complete destruction, which the human brain may sustain, consistently with sensation and voluntary motion, provided this be gradually produced, makes strongly in favour of the conjecture of Haller. " —— —— was born with a very large head, but seemed well in health, increased in strength and grew fat. The head soon became so unnaturally large and the features were so much altered, as to leave no doubt concerning the nature of the disease; the child however increased in size, grew strong in his limbs and took food: he could both hear and see well and so continued, until he was 18 months old; he then died suddenly, without any convulsive attack. On opening the cranium, more than five quarts of very limpid water were found within it, there was not the smallest trace of membrane, or brain, except opposite the orbits and meatus auditorios, where something like medulla still remained." This case was communicated to Dr. Quin by an eminent surgeon in Dublin:† It is to be wished, that it had been still more circumstantially detailed, but it appears to me allowable to infer from what is actually stated, that those extremities of the nerves, usually named their origins, were in this case at least the seat of sensation, &c.

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* Primæ Lineæ Physiologiæ § 372.

† See Quin's Treatise on the Dropsy of the Brain page 104.

2. In some animals, as the turtle and frog for example, the brain does not appear to be exclusively the seat of sensation and volition, or the power of producing voluntary motion. For, if the brain be removed, these animals on the application of a stimulus to their limbs, continue to shew indisputably by their movements, that they are possessed of sensation and volition. Here then the spinal marrow, if not the nerves, evidently participate the above powers with the brain and can exert them independently of it.*

3. Some insects and worms, when cut into two, or more pieces, become two or more distinct and perfect individuals, each possessing sensation and voluntary motion. In polypuses the nervous substance is not formed into distinct visible fibres, but distributed equally in every part of the body; and these animals may be divided into an almost infinite number of pieces, each of which becomes a distinct individual, evidently possessing sensation and voluntary motion.† For they perceive the agitation of the water, in which they are placed: They appear

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* Cuvier Anat. Comp. T. II. p. 94. † Ibid. p. 95.

sensible to heat and light and are excited to action by these stimuli.* Here then every part of the system has an equal claim to being the seat of sensation and volition.

It is only in the more complicated, or perfect animals, as M. Cuvier very justly observes, that the assemblage of the different parts of the nervous system and especially the presence of its central parts are absolutely necessary for the exercise of the functions of this system.† When we consider what takes place in polypuses, we may be led to conceive, adds this author, "that at the bottom all the parts of the nervous system are homogeneous and susceptible of a certain number of similar functions, nearly as the fragments of a large magnet, when broken, become each a small magnet, having its poles and current, and that it is accessory circumstances only and the complication of functions, which these parts have to discharge in the higher orders of animals, that render their concurrence necessary and occasion each to have a particular destination."‡

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* "L' expansion des actinies correspond parfaitement à la sérénité de l' air, le polype à bras s' apperçoit très-bien de la présence de la lumière ; il l' aime & il se dirige constamment vers elle. *Ibid.* p. 362.

† Anatom. Comparée. T. II, p. 94.

‡ Ibid. p. 95.

The wisdom and power of the Creator are, perhaps, no where more strikingly evinced than in the wonderfully diversified structure of the animal kingdom. We find an almost infinite variety in the organs, subservient to each of the numerous functions of the animal body, and yet each function performed in the manner, best adapted to the particular economy of every distinct species.

§ II.

On original Defects in the Nervous System of the Human Species.

Having pointed out generally in the preceding part of this paper the preeminence of man with respect to the proportion, organization, and functions of his nervous system, I shall now proceed to shew, that, great as the proportion of this system is, beautiful and complex as its organization is, and wonderful as its functions are in a perfect human being, his offspring, in consequence of mal-conformation, is upon some occasions unfortunately reduced to a level, with the lowest animals in the simplicity, proportion, &c, of the system under consideration.

The original defects in the nervous system of human fetuses differ considerably in their degrees; all of which I shall enumerate, beginning with the smaller ones and proceeding to the greatest.

1. Fetuses have been born, in which only a part of the brain, of greater or less extent, was wanting. In these instances the upper, or vaulted part of the skull, or at least a considerable portion of the bones, composing it, has been also found wanting. The brain is also sometimes very different from the natural state, in form, colour and consistence as well as in bulk: Dr. Monro, in one case, found the substance, occupying the place of the brain, not more bulky than a small nut and of a red colour throughout, resembling a clot of blood. The proper integuments of the skull are for the most part wanting, the degenerate portion of brain being only protected by a thin pale or reddish membrane: In one fetus, however, of this kind, of which a description and figure are given by Professor Sandifort, the covering of the brain approached more nearly to the nature of the scalp and was thinly covered with hairs.

This kind of monster, which is generally, though not very properly, styled *acephalous*, since the base of the skull and face are not wanting, is by no means an uncommon occurrence. Most accoucheurs in extensive practice have met with one or more. I have brought one into the world which was a twin of rather small size; and it is worthy of remark, that a considerable proportion of these defective fetuses has been twins.† As far as I could determine from an exterior view it was not entirely destitute of brain, nor did it present any other deformity or defect; but I had not an opportunity of examining it by dissection. Many of these monsters, however, have exhibited other kinds of deformity. A tumor, containing a thin watery fluid, has in several instances been found attached to the base of the skull at its fore, or back part—A hare-lip, sometimes with, at other times without a division of the upper jaw and palate, has been observed—The affection of the spine and spinal marrow, termed spina bifida or hydrorachitis, has been occasionally met with—An umbilical hernia of very large size has also been found—Examples of all these complicated deformities may be seen described and admirably delineated in Sandifort's Musæum Anatomicum Tab. 122, 123, 124, 126.

A fetus was lately born in Manchester, in which the brain was not only defective, but misplaced. The parietal bones and the upper part of the frontal bone were not wanting, as is generally the case in acephalous fetuses; but they were very small and pressed down flat upon the base of the skull, so that there was no room for the brain. The superior portion of the occipital bone was wanting and the canal of the spine was incomplete, being open behind from the top of the neck down to the os sacrum. The spinous processes of the vertebræ were not wanting, but appeared as if divided and turned down on each side, so that a kind of spoon-like cavity was formed, which was deepest at the upper part, owing to the bodies of the cervical vertebræ projecting forwards. In the upper part of this cavity the cerebellum was lodged and the cerebrum extended down to the os sacrum. The optic nerves arose from the base of the brain pretty low down; they were consequently much elongated; they were also much slenderer than usual, though the retina was as large and pulpy as is natural. The spinal marrow was divided into two slender cords, which were disposed one on each side of the encephalon and gave off their nerves as they descended. The nerves of the extremities were of the natural magnitude. The brain and spinal marrow had only a membranous covering posteriorly. This fetus was born dead, but not putrid; though born at the full time, it was considerably smaller than the ordinary size. It had a club foot and three of the ribs were united on the right side. The mother had perceived the motions of this fetus in the womb. Mr. Gibson dissected this singular fetus and is in possession of the skeleton.

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† The very curious acephalous monster, described by Curtius, was a twin. See *Sandifort's Thesaur. Dissert.* &c. Tom. 2. The monsters, described by Drs. Monro and Clarke, to be mentioned more particularly hereafter, were also twins.

2. The brain has been found entirely wanting, the spinal marrow being in a perfectly natural state. A remarkable case of this kind is related by Dr. Heysham, an ingenious physician at Carlisle, in his account of the Bills of Mortality of that city for the year 1788, and accompanied by some valuable remarks.

I shall give the whole of the case in his own words.

" At eight o'clock on Monday morning, May 26, 1788, Mary Clarke, aged 26 years and the mother of six children, some of whom are healthy and others unhealthy, was delivered of a living female child, at the expence of the Carlisle Dispensary. The midwife, shocked at the strange and unusual appearance of the child's head, sent for me immediately. I got there about an hour after the delivery and at first sight it appeared evident, that the bones, which form the upper part of the skull, were wanting and that the brain was only covered by its proper membranes, the pia and dura mater, and resembled a large excrescence, which projected a little over the common integuments, especially towards the forehead, where it extended over the root of the nose. The colour of this substance was a dark reddish brown, and upon examining it more particularly I thought I could perceive the division of the two hemispheres of the brain and likewise the division of the cerebrum from the cerebellum. *I gently raised with my fingers a part of it, which projected over the integuments, which made the child cry and produced a considerable starting, similar to what is occasioned by an electric shock.* The child was full grown and seemed in perfect health, her limbs were plump, firm and well proportioned, and *she moved them with apparent agility.* The external organs of sense were also perfect. *She swallowed well and took a sufficient quantity of nourishment for several days, but sometimes during the action of swallowing started a little.* She lived till five o'clock on Sunday morning, June the 1st, when she expired. Some time before her death, she was affected with slight convulsions. During the three or four days, preceding her death, there was a constant discharge of a thin watery fluid, somewhat tinged with blood, from the excrescence, which greatly diminished its bulk, for at her death it was only about half the size of what it had been, when she was born, and the surface was in some places beginning to put on an appearance of mortification."

"A few hours after her death, Dr. Blamire and Mr. Charles Farish accompanied me to the house, where Dr. Blamire very cautiously dissected away from the bones the whole of the substance; when we found the greatest part of the frontal, the temporal and the occipital, and the whole of the parietal bones wanting. The substance removed was then carefully examined, and what was our astonishment to find it entirely to consist of membranes, blood-vessels, but principally of several bags, one of which was as large as a nutmeg, the rest of different sizes, but much smaller. They were all filled with a brownish coloured fluid; which, when the *cysts* were punctured, gushed out with some violence. *There was not the least appearance of cerebrum, cerebellum, or any medullary substance whatever.* The spinal marrow had a natural appearance, but did not seem to have been connected with the parts above described."

"Having accurately related the facts, as they appeared to Dr. Blamire and myself, which for their singularity deserve to be recorded, I think the few following obvious inferences may be drawn from them. 1. That the fluid, discharged from the excrescence during the life of the infant, and which produced the greatest diminution of its bulk, was occasioned by the rupture, or erosion of *cysts*, similar to those, which remained sound and full of water after death. 2. That *the living principle, the nerves of the trunk and extremities, sensation and motion may exist independent of the brain and that the natural, vital and animal functions may be performed without the brain.* And, as the external organs of sense, viz. the eyes, the nose, the tongue and the ears, all seemed perfect, may we not therefore suppose, that the optic, the olfactory, the gustatory and the auditory nerves may exist independent of and unconnected with either the brain, or the spinal marrow?" P. 8—11.

Dr. Heysham has favoured me with an account of the following additional circumstances, relative to this accephalous infant—The eyes were as full and as lively as in any other child of the same age. The iris evidently contracted on the application of light and from other observations, which he then made, he had no doubt, that her vision was perfect. The child voided both fæces and urine in a regular and natural manner and, for the first three or four days after her birth, seemed in perfect health. No stimulants were applied to her nostrils and he does not know whether she sneezed naturally or not. As the absolute want of brain was not known, till after the child's death, he was less attentive to minute circumstances, than he would otherwise have been,

3. The brain and upper part of the spinal marrow have been wanting, where the lower portion of the latter was entire and nearly of the natural size and conformation; as in the case of the human male monster, of which Dr. Monro has given an interesting account, illustrated with engravings in the Trans. of the Royal Soc. of Edinburgh, Vol. III. p. 216 &c—This child was a twin and born at the full time. It had no head, or neck; it wanted also about one half of the ribs; the larynx, trachea and lungs; the heart; the pharynx, œsophagus and stomach with all the small intestines, except the end of the ilium; the anus; the liver, spleen, pancreas and omenta; the renal glands; terminations of the ureters; the middle part of the urethra; the right testicle; both arms; both patellæ; with several of the bones of the feet and toes"——" The spinal marrow was of a conical shape with the top, or small part of the cone at its upper end, and at its lower end it formed a cauda equina. From its two ends and sides it sent off 18 pairs of nerves; which at their origin and in their progress were nearly as large as they are in a perfect fetus, or where the brain and cerebellum are connected with the spinal marrow."

The Dr. makes the following remarks on the nervous system of this monster. " 1. As the spinal marrow and pairs of nerves, sent off from it, had nearly the usual size and structure, although the brain, cerebellum and medulla oblongata were entirely wanting, we find reason for calling in question the common doctrine of authors, which teaches that the spinal marrow and nerves derive their origin from the brain and cerebellum and are dependent upon it, as much as the ducts of glands are upon the glands, which send liquors into them. 2. Further, as the several parts of this monster were furnished with nerves, and as we have found, that its arteries and veins, by a well-regulated, varied and complicated action, circulated the blood, we must suppose, that their muscular fibres were actuated by those nerves. We therefore find in this monster, not only the existence and common appearance of the spinal marrow and nerves connected with it, although the brain and cerebellum were wanting; but we have proof, that these, independent of the brain and cerebellum, may actuate the muscular fibres in the vessels of an animal, or that nervous energy, or fluid, as it is commonly called, is not derived from the brain and cerebellum solely; that is, we conclude, *that the nerves, as well as the brain and cerebellum, are capable of furnishing nervous energy; and that there is no more reason for believing, that the nerves are derived from the brain, than that the brain is derived from the nerves; or all the parts and* *branches of the nervous system appear to possess the general power, or office of furnishing nervous energy.*" Dr. Monro had previously attempted to establish these points in his *Observations on the Structure and Functions of the Nervous System:* He says, "for the reasons given in last section, I have long thought and endeavoured to prove, that our nerves, independent of the encephalon, possess an energy, or principle of life, which they derive from their proper pia mater and its vessels." p. 35.

4. A fetus has been found on dissection to be destitute of brain, spinal marrow, and optic nerves. This fetus was brought into the world by Mr. Barlow of Bolton, and an account of the case is given in the Medical and Physical Journal for September 1800, pages 189—191; from which I shall beg leave to present the following abstract.

The mother positively asserted that she went two months over her time, and "was not sensible, during pregnancy, of any difference from what she had been formerly accustomed to, either in her own feelings, or *in the motion of the child*, and she had had many children. The birth was marked by no particular occurrence." But this fetus was still born, which the relater of the case supposes "has always been the case, when the brain was wanting."*—" In this child," says he, "the upper part of the cranium is entirely wanting; and there remains only a thin plate of bone, covered with a doubling of membrane, in place of the cervical and the greater part of the dorsal vertebræ. This fold contained no medulla, though it exhibited, on being slit open, some slender fibrils, which might be construed into nerves. I should compare it to the proper coverings of the medulla spinalis, of a thinner texture. Lower down a displaced portion of vertebræ is shewn, which was hollow, but contained no medulla: the rest of the spine consisted of a solid column of bone, without any spinous processes. The child had besides a slight inversion of the feet and a hare-lip on the right side, in other respects it was full grown and the colour of the skin was natural. There did not appear to be any deviation from the common structure and arrangement in the viscera of the thorax and abdomen."—" Though the eyes were outwardly well formed, I could not find by dissection any optic nerve. The nerves in the upper and lower extremities were, nevertheless, perfect."

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* " Infantes in lucem eduntur omni fere cerebro atque spinæ medulla destituti, qui haud plantarum ratione tantummodo aluntur atque pinguescunt; sed etiam vociferantur atque sugunt," *Soemmerring*. T. IV. p. 158.

The conclusions, which this writer draws from the case just related, are diametrically opposite to those of Prof. Monro and Dr. Heysham. They are, moreover, so curious as to deserve being noticed here. He says, "In comparing the defective structure of this child with the ascertained uses in others of those parts, of which it was deprived, I have been led to conclude that *nervous influence is not at all necessary to the growth of the fetus in utero*"—And again, "Assuming then that the nerves serve merely to convey the influence of the brain and medulla spinalis, it is obvious, that when deprived of these sources, they can impart none. Thus it is evident, that, although *this fetus had attained the full size and its motions were not perceptibly different from another*, yet, having no sensorium, it could possess no sensation."*

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* This writer also endeavours to prove, that *the perfect fetus in its uterine state does not possess sensation*, by stating that sensibility is not only unnecessary during the fetal state, but would expose the fetus to hazards, and by intimating, that sensation is coeval with respiration—I have in a former work satisfactorily shewn, that the fetus in utero most assuredly does possess both the powers of sensation and voluntary motion.

I shall, therefore, confine myself here to the following observations.

1. The proportional bulk of the nervous system is as great and its organization as complete in the fetus, as in the new-born infant.

5. A fetus has occurred, in which not only the brain and spinal marrow were wanting, but in which there was no evident appearance of nerves. A case of monstrosity of this kind is related by Dr. Clarke in the Philosophical Transactions for 1793. P. II. p. 154 &c, and accompanied by two engravings. This monster was included in a distinct set of membranes and had a placenta belonging to it, the side of which was attached to the placenta of the perfect twin. It was covered with the common integuments; was of an oval

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2. The communication betwixt the different parts of the nervous system is as free in the fetus in utero as after birth; the brain, spinal marrow and nerves being neither under the influence of pressure, nor of the action of any narcotic or other power, which can diminish the nervous energy.

3. The fetus in utero gives every indication of its possessing sensation and voluntary motion, which a being, so situated, could possibly be expected to give.

4. In whatever manner the respiration of the infant contributes to its power of sensation after birth, in the same manner does the respiration of the mother contribute to the sensibility of the living fetus in utero.

5. If we grant, that the fetus, born at Bolton, from having no sensorium could have no sensation, no argument can be drawn from thence against the power of sensation in a perfect fetus in utero; because a perfect fetus does possess sensation after birth and, agreeably to the above assumption, this defective fetus must, had it been born alive, have remained for ever destitute of sensation.

figure, about four inches in length and three inches in breadth. One edge of it was rather more concave than the other and near its centre there was a slender funis umbilicalis, about 1½ inch long, which had one artery and one vein. There were two imperfect feet and a small projection having the appearance of a finger. Internally this monster was composed of soft and bony matter; the former "appeared of a homogeneous fleshy texture, but without any regular or distinct arrangement of muscular fibres and was very vascular throughout; the bones, which were surrounded by this fleshy substance, were the os innominatum, the os femoris, the tibia, the fibula."—The os innominatum and os femoris were both perfect and as large as those of a fetus at the full period of utero-gestation. "There was not the smallest appearance of head, or vertebræ, or ribs. There was neither brain, spinal marrow, nor nerves. It had no heart, nor lungs." It contained none of the viscera subservient to digestion, except a little portion of small intestine, which had a peritoneal covering and was very vascular. It had no organs of digestion, nor any glandular substance whatever —Amongst other remarks, Dr. Clarke observes "that the deficiency of nerves renders it extremely probable, that their use is very small, if any to the embryo."

6. Dr. Clarke informs us in the same paper, "that in other cases, where the brain has been perfect, the spinal marrow has been deficient in a great part of its extent and sometimes throughout" p. 159: But I do not recollect an instance of this kind and Soemmerring says:—"Non raro spinæ medulla est sine cerebro, *numquam* vero cerebrum sine spinæ medulla est visum, deficiente enim spinæ medulla, cerebrum simul abfuit."*

§ III.

On the influence of original defects in the nervous system of man on sensation and voluntary motion.

From considering merely the laws of sensation and voluntary motion, mentioned above, as obtaining in a perfectly formed human being, a person might be led to conclude *a priori*, that a fetus, having no brain, or neither brain nor spinal marrow, must necessarily be destitute of sensibility and incapable of throwing into action any of the muscles subservient to voluntary motion.

However, when he considers, that the human brain has in various instances been very extensively injured and diseased, that it has been nearly, perhaps wholly, destroyed as in the very extraordinary case, copied above from Dr. Quin's Treatise on the Dropsy of the Brain,[†] without destroying sensation or voluntary motion, some doubts must, I think, arise in his mind as to the justness of such an inference, because he will perceive, that the nervous system of man in its entire and diseased states is governed by different laws.

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* De Corporis Humani Fabrica T. IV. p. 89: where he refers to HUBER de *Medulla Spinali*. Götting. 1789. p. 4.

Again, when he reflects upon the amazing diversity of structure, proportion, disposition &c, observed in the nervous systems of different orders of animals, from man to a zoophyte, and attends carefully to the various tenures, on which sensation and voluntary motion are held by the animal creation; he will find further reason for questioning the propriety of the ground, on which the possession of these two important faculties is denied to a human fetus, unfortunately labouring under the privation specified above: For he will be led to suppose, that its nervous system may be governed by the same laws as that of the inferior animals, which it more nearly resembles than the nervous system of its own species in the natural state.

[†] See page 489.

When he finds a mother, describing the motions of a fetus in utero, which happens to be still-born and is, after its birth, proved by dissection to have neither brain, nor spinal marrow, as exactly similar to those of her former perfect children, he surely can entertain no doubt, that this fetus, when alive, possessed the same power of voluntary motion: And, as this faculty, as well as sensation, is derived from the nervous system, he will see a strong reason for believing, that this same fetus also possessed sensibility.

Further, when he finds one of these defective beings ushered into the world alive and exhibiting, as far as can be determined, the same powers, which a perfect child of the same age displays, crying when touched rudely, moving its limbs with agility and swallowing food, living more than five days, and then dying with an incipient mortification of the head, he cannot, I apprehend, reasonably withhold his assent to this child's possessing the faculty of sensation as well as that of voluntary motion—Who will contend, that this child could not feel, because it had no sensorium commune? Who does not perceive, that the encephalon and sensorium commune are not exactly synonymous terms and consequently not always to be used indiscriminately in speaking of the animal faculties?

In one of these two cases I have no doubt, that the powers of sensation and voluntary motion were derived from the nerves independently of the brain, and in the other case, independently both of the brain and spinal marrow.

But what shall we say of the monster, described by Dr. Clarke in the Philosophical Transactions? The Dr. is a good anatomist and appears to have dissected this rude mass with great care and attention, yet he could not discover a single nerve. By a nerve, strictly speaking, is understood a continuation of the substances of the brain, or spinal marrow, wrapped in its proper membrane. By nervous matter I understand the substances of the brain, spinal marrow, or nerves, separate from their proper membranes. Now, shall we suppose that in this being no nerves, or nervous matter existed? Or, shall we suppose, that there were nerves, or nervous matter, but distributed in such a manner as to elude Dr. Clarke's observation, and that the limited powers, possessed by this rude animal production during its life, were ascribable to nervous influence?

I am inclined to adopt the latter opinion, for the following reasons.

1. Nervous and muscular fibrils may be detected by magnifying glasses in parts, where they can not otherwise be perceived, and it does not appear, that Dr. Clarke availed himself of any instrument of this kind in his search for nerves.

2. The cineritious and medullary substances are the essential parts of the nervous system, the coats or membranes of the encephalon, spinal marrow and nerves being given them merely for protection and not contributing directly either to sensation, or motion.

3. Nervous matter is distributed to many points, where we cannot demonstrate its presence to the eye and where we can only infer its existence from their sensibility. We cannot prick the skin with the smallest instrument, without exciting pain, whence it is evident, that there is nervous matter in every point of the skin, though we have no other means of proving it.

4. The blood vessels had evidently possessed the power of circulating the blood in this monstrous fetus and, though it was much inferior in size to its fellow-twin, its growth had proceeded to a certain extent, and bone, skin, cellular membrane, ligament, cartilage, intestine, &c had been formed: Now I do not admit any power as capable of giving energy to the muscular fibres of the arteries and veins, except the *vis nervea*, or nervous power.

I am of opinion, that no such power exists in muscular fibres as a *vis insita*, or inherent power, distinct from, or independent of, a nervous power. The experiments and arguments, adduced by

Dr. Monro,[*] seem to prove fully that the contraction of a muscle, which has been attributed to this supposed vis insita, may be equally well accounted for from the nervous power alone, and that the former would be a superfluous power in the animal economy, the supposed vis insita being excited, or destroyed by the same means as the nervous power. When the brain or spinal marrow is irritated, the muscles or muscular fibres contract, tremble, or are convulsed; when the point of a needle or other sharp body, is pushed into a nerve, distributed to a particular muscle, a contraction of that muscle ensues; in which cases the muscular fibres act confessedly by virtue of their nervous power. When a needle is pushed into the fibres of the muscle itself, a contraction also takes place, and in this case it has been supposed by the celebrated Haller and others, that the fibres contract by virtue of their vis insita: But the only difference appears to consist in this; that in the former experiments the stimulus acts upon the brain, spinal marrow or the trunk, or an evident branch, of a nerve, whilst in the latter it acts upon one, or more of those nervous filaments, or portions of nervous matter, which, though not always demonstrable to the sight, we have every reason to believe are constantly and invariably distributed to every muscular fibre.—It has been urged, as an argument in favour of a vis insita, that muscles, or muscular fibres, will contract for a considerable time, after their separation from the body, on the application of a stimulus.* But this fact may be perfectly explained by the admission of a nervous power only; for why cannot the nerves, which evidently possess an energy independently of the brain, retain this energy after the removal of muscles from the body, as long as the muscular fibres can retain their supposed vis insita?

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[*] Observations on the Nervous System, p. 91—94.

From what has been stated in the preceding part of this paper, the following conclusions amongst others may, I conceive, be very fairly drawn.

1. That every perfect animal, from man to the polypus, possesses the powers of sensation and voluntary motion.

2. That infants, though born destitute of brain, or even of brain and spinal marrow, possess these two important faculties.

3. That the fetus in utero is neither destitute of sensation, nor voluntary motion.

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* In warm-blooded animals this takes place for an hour or more; in cold-blooded ones more than a day afterwards.

4. That the power of action in the arteries and veins, by which the circulation of the blood and the formation of the different parts are effected in the most defective human monsters, is derived from a nervous energy, independently of brain, spinal marrow, or even evident and distinct nerves.

Experiments and Observations on the HEAT *and* COLD *produced by the* MECHANICAL CONDENSATION *and* RAREFACTION *of* AIR.

BY JOHN DALTON.

READ JUNE 27, 1800.

If a thermometer be inclosed in a receiver and the air suddenly condensed, the thermometer rises a few degrees above the temperature of the atmosphere; and if the air be exhausted from a receiver inclosing a thermometer, the mercury sinks a few degrees immediately; but in both cases after some time it resumes its former station. These facts are well known to philosophers of the present age, but they do not all agree in the explanation of them. Thinking the subject worthy of elucidation, I was induced to institute a series of experiments for the purpose, which I apprehend have led to a clear demonstration of the cause of the phenomena, and moreover make the facts themselves appear in a somewhat different point of view from what they are seen in at the first moment.

One circumstance is very remarkable, that whether the mercury rises or falls in these instances, it is done *very rapidly;* whereas in the open air, if a thermometer be only two or three degrees above or below the temperature, it moves very slowly. This seems to have suggested to every one the idea that the elasticity of the glass bulb of the thermometer has a principal share in producing the effect, by causing the bulb to yield a little to the pressure of the air. It has however been found upon trial that the same effects take place whether the thermometer is sealed or not. My experiments accord with this, having made a thermometer and left it unsealed for the express purpose; in all the experiments with condensed and rarefied air, there was no sensible difference observed to arise from the inequality of pressure on the external and internal surfaces of the bulbs, the sealed and open thermometers varying the same in kind and also in degree, except from circumstances to be noticed hereafter.

It being certain then that a real change of temperature takes place, it remained to determine the quantity and manner of that change. Having chosen a small and consequently sensible thermometer, with a scale of degrees sufficiently large to admit of distinguishing one tenth of a degree, I proceeded to ascertain several facts experimentally.

EXPERIMENT 1.

Took a receiver, the capacity of which was about 120 cubic inches, and suspended the thermometer with its clear bulb in the central part of it; then letting the whole acquire the temperature of the room, which was without a fire, I exhausted the air and afterwards restored it, marking the effects upon the thermometer. The medium of several trials nearly agreeing with each other was as under:

The Thermometer

———in the air of the room stood at....36°.8
———sunk upon exhaustion to ....34 .7
———rose when the air was restored to..38 .9

The *suddenness* of the fall and rise puzzled me most: after reflecting upon it for some time, I conjectured that the real change of temperature of the air or medium was much greater than the thermometer indicated, but that the inequality existed only for a few seconds of time, because the receiver, &c. immediately impart heat to or abstract it from so small a quantity of air as 120 cubic inches, which are only equal to 40 grains in weight.—The phenomena of the thermometer seemed very well to accord with the supposition of *great heat or cold* acting upon it for a few seconds only.

EXPERIMENT 2.

Pursuing this idea I imagined that if two thermometers whose bulbs were very unequal in magnitude were inclosed together, the smaller bulb ought to give the greater variation: accordingly I inclosed two, the diameters of their bulbs being .35 and .65 of an inch respectively; and having exhausted the air and restored it again repeatedly in succession, and found a mean of the variations, that of the small bulb was 2°. 8, and that of the large, 2°. 2.

EXPERIMENT 3.

Repeated the exhaustion with the small thermometer inclosed in three different circumstances successively; 1st with the bulb in the centre of the receiver; 2d with the bulb resting on the wet leather of the plate; and 3d with the bulb resting against the side of the receiver.

1st Case—Reduced by exhaustion	2°.45
2d Case—	2 .15
3d Case—	1 .2
1st Case—Raised by restoring the air	4 .05
2d Case—	2 .25
3d Case—	2 .8

EXPERIMENT 4.

Inclosed a wine glass with about a cubic inch of water in it, containing the bulb of a thermometer, in a receiver; and, exhausting the air, the thermometer sunk half a degree suddenly, and then continued stationary; upon restoring the air it suddenly rose half a degree.

All these experiments confirmed my conjecture of a much greater degree of heat and cold being produced in these cases than the thermometer points out, but that its continuance is so short as not to effect a material change in the temperature of the mercury. The following experiments were made to ascertain what may be the *real* degree of heat and cold generated in those operations.

EXPERIMENT 5.

The same receiver & small thermometer as above being used, I found the exhaustion was effected by working the pump *one minute.* The thermometer sunk nearly 2° in the first half minute, and the remainder, a few tenths of a degree, in the latter half minute. The operation being stopped, and things remaining in the same state, it required some minutes of time before the thermometer recovered *one* degree of the heat lost. Upon opening the cock, the receiver filled with air in five seconds, and the greatest velocity of the rising mercury was about the end of that time. The rising continued for 30 or 40 seconds from its commencement, but ¾ of the effect were produced in the first 10 seconds. The greatest velocity of the rising mercury is 1° in 3½ seconds. After the thermometer had attained its utmost height, it began to fall again at the rate of ⅒ of a degree in a minute.

EXPERIMENT 6.

Took the same thermometer and heated it to 50° above the temperature of the air, then let it be cooled by the medium of air, and it began to fall at the rate of 1° in 3½ seconds.

The two last experiments seem to prove that when air is let in to the receiver in the ordinary way, *an increase of temperature of 50° is produced in the medium within the receiver for 3½ seconds.* This high temperature is reduced *in a few seconds* by the receiver and surrounding bodies, to their own temperature.

EXPERIMENT 7.

On condensed Air.

Took a large spherical glass receiver, the capacity of which was something more than twice that of the former (above one gallon), and suspended a thermometer in the centre of it, of a larger bulb than that before used; the receiver had a brass cap and stop-cock adapted to it: Then doubled the density of the air within it by a condenser. The thermometer rose 2° or more. Let out the air suddenly and the thermometer immediately sunk each time from 3° to 3°. 5; at the same time an exceedingly dense mist was produced in the receiver, which soon subsided.

Suspecting that *aqueous vapour*, which always exists in the atmosphere, and is liable to assume the liquid or aërial form according to circumstances, might be the principal agent in the production of heat and cold by condensation and rarefaction, I thought that an increase of it might produce a greater effect, and that cold air, which contains less vapour, might have a less effect. The reverse however was the fact, as appears by the following,

EXPERIMENTS 8 & 9.

In a cold morning last winter when the air was clear and the thermometer without stood at 20°, I took the receiver and condenser into the open air, and let them stand for 15 minutes to acquire its temperature; then repeatedly condensed the air to a double density, and suddenly liberated it again. On a medium of 5 trials the mercury fell 3°.3 on opening the cock.—The vapour precipitated was whiter than usual and not nearly so dense.

Again, took the receiver and condenser into a dyer's stove where the temperature was about 100°, and the air abounded with vapour in a transparent state: after some time, condensed the air and liberated it as before, when on a medium of 5 trials the mercury sunk only 3°, and a very copious mist was precipitated, so dense that one could but just distinguish the degree of the thermometer through it.

These experiments shew that the greater the quantity of vapour condensed the less is the change of temperature; and that consequently, if air was entirely free from vapour, the change of temperature would be a *maximum*. Indeed this is clearly consistent with the known law, that when vapour is condensed, heat is given out. Any process to cool the air must be retarded by the condensation of part of the vapour it contains. Suppose for instance that a portion of the atmosphere contained $\frac{1}{60}$ of its weight of aqueous vapour, and that $\frac{3}{5}$ of this vapour were condensed by 50° of cold; that is, $\frac{1}{100}$ of the whole elastic mass was converted into water; then the heat given out would be sufficient to raise the temperature of the remaining mass of air and vapour 6 or 8°, which sufficiently accounts for the small difference observed in the results upon warm vapoury air and cold dry air. Hence vapour, far from producing the change of temperature in question, tends to diminish the effect.

If any doubt remained with me respecting the *real* change of temperature that takes place in the operations related above, it was completely removed by the results of the two following experiments.

EXPERIMENT 10.

Inclosed a small graduated glass tube of $\frac{1}{15}$ of an inch internal diameter, & 10 inches long, with a short column of mercury in it, in the large receiver; the tube was sealed at one end and open at the other, so that a portion of air of given capacity was confined by the mercurial column, which was near the open end of the tube, and subject to rise or fall by any variation of elasticity of the air on either side, being a proper manometer: then doubled the density of the air in the receiver, and opening the stop-cock, the mercurial column soon ran up to its former station, but instantly turning the cock again, the mercurial column returned or fell down gradually for 5 or 10 seconds, to the amount of nearly $\frac{1}{10}$ of the whole aërial column, and then became stationary. Again opening the cock, a quantity of air rushed out, and the mercury resumed its original station. These effects were always the same, on a repetition of the experiment.

EXPERIMENT 11.

Let the mercurial column of the manometer down by a wire to $\frac{1}{4}$ of the length of the tube from the sealed end; then exhausted $\frac{1}{4}$ of the air from the receiver, which was seen by the mercury rising to the top of the tube; and upon opening the cock the mercury fell to its former station, but then suddenly turning the cock, the mercury gradually rose for the space of 5 or 10 seconds to *more* than $\frac{1}{10}$ of its original height above the stationary point, and remained there till the cock was opened; after which it resumed its proper station.

The phenomena in the two last experiments can be explained only on the following principle:—The air in the receiver and in the manometer is subject to a like degree of rarefaction and condensation in those experiments, or very nearly so. When the equilibrium of heat in the air is disturbed by the operations of condensation and rarefaction, it is restored in the manometer *instantly* by reason of the contiguity of the glass to the air; but in the large receiver it requires a sensible time of 10 seconds or more to restore the equilibrium throughout the whole internal capacity. It is this restoration that increases or diminishes the elasticity of the air confined in the receiver, and thereby causes the retrogradation of the mercurial column. Now I have found by former experiments that a change of 50° in temperature effects a change of $\frac{1}{10}$ nearly, in the capacity or bulk of air. It follows therefore that in the case of restoring the equilibrium in condensed air, about 50° of cold is produced; and in letting in air to an exhausted receiver something more than 50° of heat is produced. The small difference seems to arise from this, that the condensation of vapour in the former case *diminishes* the effect, and in the latter, if any there be, *increases* the effect, that would arise from operating upon purely dry air.

The experiments and observations hitherto related go principally to ascertain facts without any reference to the theory of them: This however may be given in a few words, and is the same that is ascribed to Mr. Lambert by Messrs. Saussure and Pictet and by them adopted. He conceives that a vacuum has its proper capacity for heat, the same as air, or any other substance; and that the capacity of a vacuum for heat is *less* than that of an equal volume of atmospherical air; also that the *denser* air is, the *less* is its capacity for heat: upon these principles the phenomena are easily referable to that class of chemical facts where heat and cold are generated by the mixture of two different bodies.—If this theory be right, and I think there is little doubt of it, we may hence be led into a train of experiments, by which the absolute capacity of a vacuum for heat may be determined; and likewise the capacities of the different gases for heat, by a method wholly new:—but this must be left to future investigation.

ACCOUNT of some *ANTIQUES*, LATELY FOUND IN THE RIVER RIBBLE, &c.

BY MR. THOMAS BARRIT.

READ SEPTEMBER 26, 1800.

The articles here exhibited for the amusement of the society are principally a few different specimens of antiques, generally denominated Celts, lately found in the river Ribble in this county.

As the particular uses of these celts have been very learnedly discussed and still left undetermined, little more can be advanced upon the subject than to quote the remarks our best antiquaries have made. Mr. Pegge (Archæologia, page 85, vol. 9,) says, these brazen instruments seem at present undetermined, it not being yet ascertained, whether they were for military purposes, or for civil and domestic employments; and, after all that has been advanced by writers upon the subject, it remains an undecided point. I shall not therefore attempt to resume the consideration of this question, but may venture to embrace the opportunity of making a few observations concerning them.

These celts are always of brass, let their form be ever so various, always with a thick crust of *aerugo* upon them. They have been found in many different places in England, Wales and Ireland; at Herculaneum and other places upon the Continent; and are supposed to have belonged to the Celtæ, or first inhabitants of this island, from whom the name is given by antiquaries to these instruments: in France they are called Gallic hatchets.

They have been supposed older than the invention of iron, which perhaps may be the case from whence they came, and in Britain where they were brought; but a conclusion from this cannot be drawn that iron was unknown in other places.

Mr. Lort, in his observations upon celts, says they were too awkward to have been invented and fashioned by the Romans, and at the same time that they were too correct and shapely to have been the work of the Britons before the invasion of Julius Cæsar. All authors however have agreed to allow them to be of high antiquity: as to my opinion I am inclined to think them the greatest antiquties this island can boast of, and that they surpass all others in point of age. Their resemblance is not found upon any

Greek or Roman coin; neither are they to be found amongst those models of armour and weapons upon the Trajan or Antonine pillars at Rome; nor do any of the writers upon the Roman military art mention or describe any offensive weapons of this sort; and therefore when any have been found in undoubted Roman stations, and accompanied even with Roman coins, &c. we are obliged to suppose either that they came thither by chance as the spoils of some British or Celtic enemy, or that they were the arms or tools of barbarian auxiliaries. Sir James Ware observes, " It is past controversy the arms of the ancient Irish were made of brass, and likewise those of the ancient Greeks, Germans and Britains." Some again have thought, and with great probability, that they were introduced into this island by the Phœnician merchants of Tyre and Sidon, who had in return British tin. Whoever were the people that used them, I am inclined to believe that some of the tribes of the Indians in North America were their descendants, from the agreement observable between the ancient Celt and the modern Tomahawk, both in size and shape; and this last is used both for a weapon and for domestic purposes.

Nearly all these celts have or have had loops at the sides, beyond all doubt to tie them to a handle, yet I do not find that any writer upon the subject has had any idea of the handles being other than straight ones; which leads one to conclude the common name of battle ax to be very improperly applied, and that of pike, pilum or javelin to be more consistent. One of these, No. 6, I make no doubt was of this kind, although now without a considerable portion of its length. No. 2 is in the form of an ax, and with Nos. 1, 3, 4, 9, whose edges have the appearance of chizels, may all have been used as axes with very great propriety, by being fitted with handles bent at the end, and thereby would become formidable weapons either against man or beast; or being mounted upon a straight shaft, might be used for pushing forward or piercing the earth, as an hoeing tool, and so capable of serving the two-fold purpose of peace or war; and with the loops at the sides might be suspended by a thong over the shoulder or 'round the waist when not wanted; and, upon occasion, it was an easy matter for inhabitants of the woods, as the ancient Britons were, to break from a tree a bough with an acute angle, and immediately to accommodate himself with a utensil for his present purpose.

It seems a little strange and what I have often wondered at, that these celts are so often called battle axes, and none of our antiquaries have mentioned the idea of a crooked handle which certainly gives them a much greater importance as military weapons. It is not at all unlikely but that a certain sort of these celts might have been used by the Druids of this country, for cutting down the mizeltoe from trees, and chopping it to pieces for their mystical purposes. Medea (in one of Seneca's plays) is represented cutting roots and herbs for her inchanted chaldron with a brazen knife. Pliny says, the mizeltoe was cut down with a golden sickle, which words I apprehend being taken too literally, have been the means of leading more than one learned antiquary some years back into a mistake, causing them to introduce into engravings the representation of a Druid with a sickle in his hand, of the shape used by the farmers of the present day; but as the Druid's sickle or bill has never been described, and no weapon of the form of our present sickle, fabricated of the metal now denominated gold, has ever yet been found, we have reason to suspect the truth of the relation as to the metal, and ought rather to credit the celt to have been the sickle of the Druid, which was made of brass or copper, and perhaps from the high price, scarcity and colour, might have been esteemed precious and valuable as gold.

No. 10 was found in the summer of 1799, near Leigh in this county, and is evidently the head of a spear. The loops on each side would almost lead one to suppose it was of celtic origin; but I am inclined to think it was the head of a Roman standard, to which the silken or linen *labarum* was affixed, which was suspended from the top of a spear by means of a small yard, like the sail of a ship. This conjecture will be strongly corroborated by attending to Roman coins of the later Emperors, whereon is seen a standard of the above description, as on some of the coins of Constantines, Valens and other Emperors; these standards or colours were introduced after the metal eagle, the boar, the hand, the head of the Emperor, &c. were laid aside.

No. 11 is a lump of sal ammoniac found at the entrance of Gaythorn Row, the top of Deansgate, near Castlefield, Manchester, in the year 1788, with a Roman coin of Tetricus, who ruled in Britain under the Emperor Aurelian.* What this salt might have been used for by the ancients, if it had any use,or indeed whether it may not be a natural production, is uncertain; many fragments of unglazed pots, and one in tolerable preservation with two handles, were turned up at the same time. Perhaps upon or very near this place was the pottery to the Roman station near adjoining. Verstigan in his "Restitution of decayed Intelligence in Antiquities," when mentioning the different fossils, shells, bones, &c. found in Britain says: " Moreover potters in " working their clay, which is gotten in some " espetial places, do fynd in it certain things " which are as hard as stone, and of the very " forme and shape of the toungs of some sortes " of fishes, each with the root unto it, to make " it the very markable and right proportion of " such a kynd of toung in all respects, some " being more than two inches long, and some " lesse than one inche, and they that thus find " them do not otherwise call them but the toungs " of fishes, which being so, and turned into very " hard stone is a strange thing in nature." Whether this same article be of the sort Verstigan alludes to, cannot at this day be determined; however, from the shape and its being found in the scite of an ancient pottery, it may be supposed to be one of those he describes.

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* About the year 274.

No. 12 is a ring of brass found in Castlefield in 1796, with a bluish sort of a bead of pot ware upon it and ribbed, each rib terminating at the hole through which the ring passes; another of the same sort, supposed to belong to the other, was so broken as not to be worth gathering. I am inclined to think this ring was the bracelet of a British or Roman lady, and the beads upon it the amulet or charm to protect the wearer from misſortunes or injuries, and to procure the ſavour of lovers and ſuperiors. This idea I think to be the origin of precious ſtones being afterwards ſet in rings. Several authors as late as the fifteenth century wrote upon the virtues of ſtones, and aſcribed to them qualities which at this day every one is not inclined to believe they poſſeſs.

EXPERIMENTAL ESSAYS

On the Constitution of mixed GASES; *on the Force of* STEAM *or* VAPOUR *from Water and other Liquids in different temperatures, both in a Torricellian Vacuum and in Air; on* EVAPORATION; *and on the Expansion of* GASES *by Heat.*

BY JOHN DALTON.

READ OCT. 2, 16 AND 30, 1801.

THE progress of philosophical knowledge is advanced by the discovery of new and important facts; but much more when those facts lead to the establishment of *general* laws. It is of importance to understand that the descent of falling bodies is the same every where on the surface of the earth; but from that and some other particular facts to infer the law of gravitation, or that all matter attracts with a force decreasing as the square of the distance, is a much higher attainment in science. In the train of experiments lately engaging my attention some new facts have been ascertained, which with others, seem to authorise the deduction of general laws, and such as will have influence in various departments of natural philosophy and chemistry.

As the detail of experiments will be best understood and their application seen, if the laws of principles alluded to be kept in view, it may be proper here to state them; though it must not be understood that they were proceeded upon hypothetically in the direction of those experiments. On the contrary, the first law, which is as a mirror in which all the experiments are best viewed, was *last* detected, and after all the particular facts had been previously ascertained.

1. When two elastic fluids, denoted by *A* and *B*, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of *A* do not repel those of *B*, as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

2. The force of steam from all liquids is the same, at equal distances above or below the several temperatures at which they boil in the open air: and that force is the same under any pressure of an other elastic fluid as it is in vacuo. Thus, the force of *aqueous* vapour of 212° is equal to 30 inches of mercury; at 30° below, or

182°, it is of half that force; and at 40° above, or 252°, it is of double the force; so likewise the vapour from sulphuric ether which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force: and so in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to ½ inch of mercury, when admitted into a torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

4. All elastic fluids expand the same quantity by heat: and this expansion is very nearly in the same equable way as that of mercury; at least from 32° to 212°.—It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it; and consequently the expansion of the fluid as the cube of the temperature, reckoned from the point of total privation.

Having now stated the chief principles which seem to be established from the following series of facts and observations, I shall proceed to treat of them under the several heads.

ESSAY I.

On the Constitution of mixed Gases: and particularly of the Atmosphere.

Ever since the discovery of the atmosphere consisting of two distinguishable elastic fluids of different specific gravities, it has been a subject of insurmountable difficulty to explain clearly the mode of their combination. Two opinions have been given respecting it: the one supposes that the two fluids are merely mixed together, without any chemical combination; but assigns no reason why they do not separate and the heaviest take the lowest place. The other supposes a true chemical union to exist between the two, and thus obviates the difficulty arising from the consideration of specific gravity; but this produces others of no less magnitude. Why does no change of bulk, of temperature, or of any of their distinct properties take place, which is usual on all other chemical combinations? Why do not oxygenous and azotic gases taken in due proportion, and mixed, constitute nitric acid gas, another elastic fluid, totally distinct in its properties from either of the ingredients? To these questions, and many others that might be proposed, no satisfactory answer has ever been given. Indeed this hypothesis is much the most untenable of the two; the notion of chemical affinity connecting elastic particles mutually *repelling* each other is plainly an absurdity; and if we suppose the particles to have no repulsion to each other, but instead of it to coalesce, then it is impossible to conceive why they do not form nitric acid gas, and why a change of bulk, or temperature, or both does not occur. All these difficulties are entirely removed by the subsequent theory, for the understanding of which it will be needful to premise certain propositions.

Prop. 1. The density of elastic fluids is exactly as the compressing force, all other circumstances alike.

This is a physical proposition and depends upon experiment for proof. All experiments agree that if a quantity of air be pressed with two, three, &c. times the force of the atmosphere, it will occupy $\frac{1}{2}$, $\frac{1}{3}$, &c. of the space occupied before; and when the pressure of the atmosphere is taken off it expands accordingly. This I find is not *strictly* true with regard to atmospheric air; which, when condensed with a double force, occupies a space something *less* than what the above ratio assigns; because aqueous vapour, one of the elements of atmospheric air, loses its form, or becomes partly converted into water by pressure. If air confined by sulphuric acid be tried, it accords very exactly with the above law. If air confined by water be tried, the condensed air is always something *less* and the rarefied air something *more* than what the theory assigns, which is entirely owing to the destruction or formation of a quantity of aqueous vapour. My method of experimenting is very simple; it consists in condensing or rarefying the air by a column of mercury in a long straight tube, divided into equal portions; the tube must be $\frac{1}{12}$ or $\frac{1}{15}$ of an inch internal diameter, and then it may be inverted without losing its contents, if the mercurial column be less than 30 inches, when the rarefaction of air is the object.

Prop. 12. Homogeneous elastic fluids are constituted of particles that repel one another with a force decreasing directly as the distance of their centres from each other.

This proposition is a mathematical one, and its demonstration founded upon the fact of the density being as the pressure. The proof may be seen in the Principia, B. 2. Prop. 23. It follows too that the distances of the centres of the particles, or which is the same thing, the diameters of the spheres of influence of each particle, are inversely as the cube root of the density of the fluid.

The proposition applies to *homogeneous* elastic fluids only; how far it may apply to mixed fluids remains to be considered.—With regard to the constitution of such there may be several hypotheses; some of which we shall now consider.

1. *The particles of one elastic fluid may repel those of another with the same force as they repel those of their own kind.*

In this case, if *m* measures of *A* were mixed with *n* measures of *B*, in the pneumatic apparatus, and under the atmospheric pressure of 30 inches of mercury, the two would occupy $m+n$ measures of space. If they were of the same specific gravity, they would remain in the situation they were left, of intimate mixture or of separation, as it happened. If they were of different specific gravities, the lightest would rise to the top of the vessel:—The pressure on each particle of the mixture would be equal to 30 inches of mercury.

Now with regard to the application of these principles; as we know of no two elastic fluids, which when mixed, obey the laws of their specific gravities; this hypothesis is inconsistent with the phenomena.

2. *Particles of one elastic fluid may repel those of another with forces greater or less than what they exert upon their own kind.*

Here again m measures of A, with n measures of B would occupy $m+n$ measures, and the pressure on each particle of the mixture be the same, and equal to 30 inches of mercury. But the fluids in this case could not remain mixed or diffused intimately through each other; that fluid of the greatest specific gravity must take the lowest place.—This therefore is equally inconsistent with the known phenomena, and must be rejected also.

3. *The particles of one elastic fluid may have a chemical affinity or attraction for those of another.*

Here if m measures of A and n measures of B were mixed, a union of particles ensues, and the new compound may assume the solid, liquid or aëriform state according to its nature. If the compound be of the solid or liquid form, the two elastic fluids may wholly disappear; if it be aëriform, then a diminution of bulk, an increase of temperature and of specific gravity may be expected——Several facts in chemistry accord with this hypothesis.—When muriatic acid gas and ammoniacal gas are mixed together in due proportion, a solid substance, muriate of ammonia, is formed, and the gases wholly disappear. When ammoniacal gas and aqueous vapour are mixed, the two unite and a portion of the compound becomes liquid. When nitrous gas and oxygenous gas are mixed in due proportion, the two unite and form a new elastic compound of greater specific gravity and consequently of less bulk, nitric acid gas.—But there are other cases of mixtures of elastic fluids, some of which have been mentioned, where no signs of chemical affinity are discoverable; in regard to such this hypothesis fails equally with the other two. We must therefore have recourse to another.

4. *The particles of one elastic fluid may possess no repulsive or attractive power, or be perfectly inelastic with regard to the particles of another: and consequently the mutual action of the fluids be subject to the laws of inelastic bodies.*

According to this hypothesis if *m* measures of *A* be mixed with *n* measures of *B*, the two will occupy $m+n$ measures of space. The particles of A meeting with no repulsion from those of B further than that repulsion which as obstacles in the way they may exert, would instantly recede from each other as far as possible in their circumstances, and consequently arrange themselves just the same as in a void space; their density, considered abstractedly, becoming $\frac{m}{m+n}$, (that of the compound being supposed unity). In like manner the particles of B must recede from each other, till they become of the density $\frac{n}{m+n}$; thus the two gases become rarefied to such degree that their united forces only amount to the pressure of the atmosphere.—Here the particles of one fluid not pressing at all upon those of the other, the consideration of specific gravity does not enter. That part of the atmospheric pressure which the fluid A sustains, will be $\frac{m}{m+n}$; and the remainder, $\frac{n}{m+n}$, is the part that the fluid B sustains. The weight or pressure upon any one particle of any fluid mixture of this sort will arise solely from the particles of its own kind.

It is scarcely necessary, I think, to insist upon the application of this hypothesis to the solution of all our difficulties respecting the constitution of mixed gases where no chemical union ensues.

That moment we admit it every difficulty vanishes, and every fact appears a simple and immediate consequence of it. The atmosphere, or to speak more properly the compound of atmospheres, may exist together in the most intimate mixture, without any regard to their specific gravities, and without any pressure upon one another. Oxygenous gas, azotic gas, hydrogenous gas, carbonic acid gas, aqueous vapour, and probably several other elastic fluids may exist in company under any pressure and in any temperature, whilst each of them, however paradoxical it may appear, occupies the whole space allotted for them all. For, the space with them all in it, is little more comparatively than a vacuum ; such is the great tenuity of all elastic fluids.

I shall now proceed to make a few observations on that collected mas of elastic fluids constituting our atmosphere, the principal of which are the *azotic* atmosphere, the *oxygenous* atmosphere, the *aqueous vapour* atmosphere, the *carbonic acid* atmosphere and the *hydrogenous* atmosphere.

Before the modern discoveries in chemistry, the atmosphere was considered as *one* simple elastic fluid, *sui generis*, containing in it, by some means or other, certain foreign substances not essentially but accidentally mixed with it. Lavoisier taught us there were *two* essentially distinguishable fluids to be found in it, and certain other substances accidentally or chemically combined with them; it now appears there are at least *four* distinct elastic fluids found in every portion of atmospheric air subject to examination. And these, for aught that appears, are totally independent one of another; so much that if any one of them was wholly withdrawn from the surface of the earth, the rest would not at all be affected by the circumstance, either in their density or situation; or if an atmosphere of another kind were added to them, they would still retain their respective stations and densities, provided that added had no chemical affinity for any one of them in the common temperature.

The *azotic* atmosphere is by far the largest and densest of them all: it supports the mercury in the barometer at a medium nearly 21. 2 inches: it is the same in quantity all over the surface of the earth; because not condensible into a liquid form at any temperature found there.

The *oxygenous* atmosphere is the next in quantity; its pressure on the surface of the earth amounts to about 7. 8 inches at a medium; it is the same nearly in quantity every where, because it preserves its elasticity in all observable temperatures.

The *aqueous vapour* atmosphere is variable in quantity according to temperature; in the torrid zone its pressure on the surface of the earth is equal to the force of ,6 and from that to one inch of mercury. In these parts it rarely amounts to a pressuse of ,6, but I have frequently observed it above half an inch in summer; in winter it is sometimes so low as to be of no more force than ,1 of an inch of mercury, or even half a tenth, in this latitude, and consequently much less where the cold is more severe.* This want of equilibrium in the aqueous vapour atmosphere is a principal cause of that constant inundation of it into the temperate and frigid zones, where it be comes in part condensed in its progress by the cold, like the vapour of distillation in the worm of a refrigeratory, and supplies the earth with rain and dew.

The *carbonic acid* atmosphere has not perhaps been accurately ascertained in quantity; it is found every where in a small proportion, not being condensible into a liquid by the usual degree of cold; its pressure may probably amount to half an inch of mercury.

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* The means of ascertaining its quantity or pressure will be given hereafter.

The *hydrogenous* atmosphere is so small in quantity as scarcely to be at all appreciable; yet, as various processes on the surface of the earth disengage this gas, and as it mixes with all the other gases constituting the atmosphere without combining with any, or rising above them, we ought to find a proper *hydrogenous* atmosphere. Perhaps we have got no tests for ascertaining very small quantities of it.

Lavoisier describes the atmosphere to be "a compound of all the fluids which are susceptible of the vaporous or permanently elastic state in the usual temperature, and *under the common pressure*." This last limitation should be omitted; he seems moreover to conceive that atmospheric pressure is the cause why water retains its liquid form at the common temperature: this notion is certainly wrong; were every atmosphere, except that of aqueous vapour, instantly annihilated, little addition would be made to the aqueous atmosphere, because it already exists in every place, almost entirely up to what the temperature will admit; the evaporation of water would be essentially the same in that case as it is at present; only the full effect would take place in less time. In short this notion of pressure preventing the evaporation of liquids, which seems to have been taken as an axiom by modern philosophers, has been the cause of more error and perplexity perhaps than any other ungrounded opinion.

Lavoisier thought that in the higher regions of the atmosphere a stratum of inflammable fluid exists in which the aurora borealis and other fiery appearances are produced; this opinion is plausible enough; but that fluid cannot be hydrogenous gas, because its particles are not repulsive of those of the other atmospheric gases, as appears by its intimate and almost instantaneous diffusion amongst them.

There may be gases in the higher regions of which we have not the principles below, the whole stock of matter being spent in their formation; and being constituted of particles repulsive of those of the atmosphere, according to the first or second hypothesis, and of less specific gravity than the other gases, they must float upon the surface of the common atmosphere, and consequently for ever elude the investigation of philosophy. He observes that even *metallic* substances may be found within the regions of the atmosphere: I have myself shewn, in my Meteorological Essays, page 180, that a fluid possessing *magnetic* properties constantly holds a place in the higher regions of the atmosphere, and which therefore we cannot help considering of a *ferruginous* quality; but it will probably ever be beyond the reach of philosophical research to ascertain the nature of so subtile and distant a fluid.

ESSAY II.

On the Force of Steam or Vapour from Water and various other Liquids, both in a Vacuum and in Air.

SECTION I.

On Vapour in Vacuo.

The term *steam* or *vapour* is equally applied to those elastic fluids which, by cold and pressure of certain known degrees, are reduced wholly or in part into a liquid state. Such are the elastic fluids arising from water, alkohol, ether, ammonia, mercury, &c. Other elastic fluids that cannot be reduced, or rather that have not yet been reduced, into a liquid state by the united agency of those two powers, are commonly denominated *gases*. There can scarcely be a doubt entertained respecting the reducibility of of all elastic fluids of whatever kind into liquids; and we ought not to despair of effecting it in low temperatures and by strong pressure exerted upon the unmixed gases. However unessential the distinction between the gases and vapours may be in a chemical sense, their *mechanical* action is very different. By increasing the quantity of any gas in a given space the force of it is proportionally increased; but increasing the quantity of any liquid in a given space does not at all affect the force of the vapour arising from it. On the other hand, by increasing the temperature of any gas a proportionate increase of elasticity ensues; but when the temperature of a liquid is increased, the force of vapour from it is increased with amazing rapidity, the increments of elasticity forming a kind of geometrical progression, to the arithmetical increments of heat.—Thus, the ratio of the elastic force of atmospheric air of 32° to that at 212°, is nearly as 5 : 7; but the ratio of the force of aqueous vapour proceeding from water of 32° and 212°, is as 1 : 150 nearly.

The object of the present essay is to determine the utmost force that certain vapours, as that from water, can exert at different temperatures. The importance hitherto attached to this enquiry has arisen chiefly from the consideration of steam as a mechanical agent; and this has directed the attention more especially to high temperatures. But it will appear from what follows that the progress of philosophy is more immediately interested in accurate observations on the force of steam in low temperatures. Different authors have published accounts of their experiments on the force of steam: I have on a former occasion (Meteorological Essays, page 134) given a table of forces for every 10° from 80° to 212°. The author of the article "Steam" in the Encyclopedia Britannica, has done the same from 32° to 280°: and M. Betancourt, in the "Memoirs des sçavans etrangeres" for 1790, (see Hutton's Math. Diction. page 755) has given tables on the subject, both for vapour from water and spirit of wine, also from 32° to 280°. But these two authors, having assumed the force of vapour from water of 32° to be nothing, are essentially wrong at that point and in all the lower parts of the scale; and in the higher part, or that above 212°, they determine the force too much; owing as I apprehend to a quantity of air, which being disengaged from the water by heat and mixing with the steam, increases the elasticity.—In a question of such moment it seemed therefore desirable to obtain greater accuracy.

My method is this: I take a barometer tube perfectly dry, and fill it with mercury just boiled, marking the place where it is stationary; then having graduated the tube into inches and tenths by means of a file, I pour a little water (or any other liquid the subject of experiment) into it, so as to moisten the whole inside; after this I again pour in mercury, and, carefully inverting the tube, exclude all air: the barometer by standing some time exhibits a portion of water, &c. of $\frac{1}{8}$ or $\frac{1}{10}$ of an inch upon the top of the mercurial column; because being lighter it ascends by the side of the tube; which may now be inclined and the mercury will rise to the top manifesting a perfect vacuum from air. I next take a cylindrical glass tube open at both ends, of 2 inches diameter and 14 inches in length; to each end of which a cork is adapted, perforated in the middle so as to admit the barometer tube to be pushed through and to be held fast by them; the upper cork is fixed two or three inches below the top of the tube and is $\frac{1}{2}$ cut away so as to admit water, &c. to pass by; its service being merely to keep the tube steady. Things being thus circumstanced, water of any temperature may be poured into the wide tube, and thus made to surround the upper part or vacuum of the barometer, and the effect of temperature in the production of vapour within can be observed from the depression of the mercurial column. In this way I have had water as high as 155° surrounding the vacuum: but as the higher temperatures might endanger a glass apparatus; instead of it I used the following:—

Having procured a tin tube of 4 inches in diameter and 2 feet long, with a circular plate of the same soldered to one end having a round hole in the centre, like the tube of a reflecting telescope, I got another smaller tube of the same length soldered into the larger, so as to be in the axis or centre of it: the small tube was open at both ends, and on this construction water could be poured into the large vessel to fill it, whilst the central tube was exposed to its temperature. Into this central tube I could insert the upper half of a syphon barometer, and fix it by a cork, the top of the narrow tube also being corked: thus the effect of any temperature under 212° could be ascertained, the depression of the mercurial column being known by the ascent in the exterior leg of the syphon.

The force of vapour from water, between 80 and 212° may also be determined by means of an air-pump; and the results exactly agree with those determined as above. Take a Florence flask half filled with hot water, into which insert the bulb of a thermometer; then cover the whole with a receiver on one of the pump plates, and place a barometer gage on the other: the air being slowly exhausted, mark both the thermometer and barometer at the moment ebullition commences, and the height of the barometer gage will denote the force of vapour from water of the observed temperature. This method may also be used for other liquids. It may be proper to observe the various thermometers used in these experiments were duly adjusted to a good standard one.

After repeated experiments by all these methods, and a careful comparison of the results, I was enabled to digest the following table of the force of steam from water in all the temperatures from 32° to 212°.

Two important enquiries still remained: the first, to determine the force of steam from water above 212° and below 32°; the second, to determine the comparative forces of vapour from other liquids. These enquiries seemed independent of each other; notwithstanding which I found them in reality connected.

Upon examination of the numbers in the table within the limits just mentioned, there appears something like a geometrical progression in the forces of vapour; the ratio however, instead of being constant, is a gradually diminishing one: thus, the force at 32° = ,200 inch.

$$\begin{array}{rl} \text{—— at } 122^\circ = 3.\ 500 & \left.\begin{array}{r} 17\cdot\ 50 \\ 8.\ 57 \end{array}\right\}\text{Ratios} \\ 212^\circ = 30.\ 000 & \end{array}$$

If we divide these ratios, according to observation, they will stand thus:

Force at 32° =	,200 inch.		
		4. 550	
77° =	,910		
		3. 846	
122° =	3. 500		Ratios
		3. 214	
167° =	11. 250		
		2. 666	
212° =	30. 000		

If we divide these again, they become;

Force at 32° =	,200 inch.		
		2. 17	
54°½ =	,435		
		2, 09	
77° =	,910		
		2. 00	
99°½ =	1. 820		
		1. 92	
122° =	3. 500		Ratios
		1. 84	
144°½ =	6. 450		
		1. 75	
167° =	11. 250		
		1. 67	
189°½ =	18. 800		
		1. 59	
212° =	30. 000		

By another division we obtain the ratios for every 11°¼ of temperature from 32° to 212° as under:

			Ratios
Force at	32° =	,200 inch.	
			1. 485
	43¼ =	,297	
			1. 465
	54½ =	,435	
			1. 45
	65¾ =	,630	
			1. 44
	77 =	,910	
			1. 43
	88¼ =	1. 290	
			1. 41
	99½ =	1. 820	
			1. 40
	110¾ =	2. 540	
			1. 38
	122 =	3. 500	
			1. 36
	133¼ =	4. 760	
			1. 35
	144½ =	6. 450	
			1. 33
	155¾ =	8. 550	
			1. 32
	167 =	11. 250	
			1. 30
	178¼ =	14. 600	
			1. 29
	189½ =	18. 800	
			1. 27
	200¾ =	24. 000	
			1. 25
	212 =	30. 000	

Thus it appears that a ratio having a uniform decrease nearly takes place; and we may therefore extend the table of forces at both extremes, without the aid of experiment, to a considerable distance. Thus, assuming the ratios for each interval of 11°¼ below 32° to be, 1. 500, 1. 515, 1. 530, 1. 545, &c. and for each interval above 212° to be 1. 235, 1. 220, 1. 205, 1. 190, 1. 175, 1. 160, 1. 145, 1. 130, &c. we can extend the table many intervals of temperature, and determine all the intermediate degrees by interpolation. This method may be relied upon as a near approximation; however it does not supersede the expediency of determination by experiment; though that is much more difficult above 212°, and below 32°, than in the intermediate degrees: because it is difficult to procure a steady heat above 212°; and below 32° the variation of force becomes so small as to elude minute discrimination. It will appear from what follows that the extension of the table by this method above 212° is in all probability accurate, or very nearly so, for 100° or more.

TABLE

Of the Force of Vapour from Water in every temperature from that of the congelation of Mercury, or 40° below zero of Fahrenheit, to 325°.

Temperature. °	Force of Vap. in inches of Mercury.	Temperature. °	Force of Vap. in inches of Mercury.	Temperature. °	Force of Vap. in inches of Mercury.
-40	,013	15	,108	34	,214
-30	,020	16	,112	35	,221
-20	,030	17	,116	36	,229
-10	,043	18	,120	37	,237
		19	,124	38	,245
0	,064	20	,129	39	,254
1	,066	21	,134	40	,263
2	,068	22	,139	41	,273
3	,071	23	,144	42	,283
4	,074	24	,150	43	,294
5	,076	25	,156	44	,305
6	,079	26	,162	45	,316
7	,082	27	,168	46	,328
8	,085	28	,174	47	,339
9	,087	29	,180	48	,351
10	,090	30	,186	49	,363
11	,093	31	,193	50	,375
12	,096			51	,388
13	,100	32	,200	52	,401
14	,104	33	,207	53	,415

TABLE CONTINUED.

Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap in inches of Mercury	Temperature.	Force of Vap. in inches of Mercury.
54°	,429	82°	1. 07	110°	2. 53
55	,443	83	1. 10	111	2. 60
56	,458	84	1. 14	112	2. 68
57	,474	85	1. 17	113	2. 76
58	,490	86	1. 21	114	2. 84
59	,507	87	1. 24	115	2. 92
60	,524	88	1. 28	116	3, 00
61	,542	89	1. 32	117	3. 08
62	,560	90	1. 36	118	3. 16
63	,578	91	1. 40	119	3. 25
64	,597	92	1. 44	120	3. 33
65	,616	93	1. 48	121	3. 42
66	,635	94	1. 53	122	3. 50
67	,655	95	1. 58	123	3. 59
68	,676	96	1. 63	124	3. 69
69	,698	97	1. 68	125	3. 79
70	,721	98	1. 74	126	3. 89
71	,745	99	1. 80	127	4. 00
72	,770	100	1. 86	128	4. 11
73	,796	101	1. 92	129	4. 22
74	,823	102	1. 98	130	4. 34
75	,851	103	2. 04	131	4. 47
76	,880	104	2. 11	132	4. 60
77	,910	105	2. 18	133	4. 73
78	,940	106	2. 25	134	4. 86
79	,971	107	2. 32	135	5. 00
80	1. 00	108	2. 39	136	5. 14
81	1. 04	109	2. 46	137	5. 29

TABLE CONTINUED.

Temperature.	Force of Vap: in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
138°	5.44	166°	10.96	194°	20.77
139	5.59	167	11.25	195	21.22
140	5.74	168	11.54	196	21.68
141	5.90	169	11.83	197	22.13
142	6.05	170	12.13	198	22.69
143	6.21	171	12.43	199	23.16
144	6.37	172	12.73	200	23.64
145	6.53	173	13.02	201	24.12
146	6.70	174	13.32	202	24.61
147	6.87	175	13.62	203	25.10
148	7.05	176	13.92	204	25.61
149	7.23	177	14.22	205	26.13
150	7.42	178	14.52	206	26.66
151	7.61	179	14.83	207	27.20
152	7.81	180	15.15	208	27.74
153	8.01	181	15.50	209	28.29
154	8.20	182	15.86	210	28.84
155	8.40	183	16.23	211	29.41
156	8.60	184	16.61	212	30.00
157	8.81	185	17.00		
158	9.02	186	17.40	213	30.60
159	9.24	187	17.80	214	31.21
160	9.46	188	18.20	215	31.83
161	9.68	189	18.60	216	32.46
162	9.91	190	19.00	217	33.09
163	10.15	191	19.42	218	33.72
164	10.41	192	19.86	219	34.35
165	10.68	193	20.32	220	34.99

TABLE CONTINUED.

Temperature.	Force of Vap in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.	Temperature.	Force of Vap. in inches of Mercury.
221°	35. 63	249°	57. 31	277°	85. 47
222	36. 25	250	58. 21	278	86. 50
223	36. 88	251	59. 12	279	87. 63
224	37. 53	252	60. 05	280	88. 75
225	38. 20	253	61. 00	281	89. 87
226	38. 89	254	61. 92	282	90. 99
227	39. 59	255	62. 85	283	92. 11
228	40. 30	256	63. 76	284	93. 23
229	41. 02	257	64. 82	285	94. 35
230	41. 75	258	65. 78	286	95. 48
231	42. 49	259	66. 75	287	96. 64
232	43. 24	260	67, 73	288	97. 80
233	44. 00	261	68. 72	289	98. 96
234	44. 78	262	69. 72	290	100. 12
235	45, 58	263	70. 73	291	101. 28
236	46. 39	264	71. 74	292	102. 45
237	47. 20	265	72, 76	293	103. 63
238	48. 02	266	73. 77	294	104. 80
239	48. 84	267	74. 79	295	105. 97
240	49. 67	268	75. 80	296	107. 14
241	50. 50	269	76. 82	297	108. 31
242	51. 34	270	77. 85	298	109. 48
243	52. 18	271	78. 89	299	110. 64
244	53. 03	272	79. 94	300	111. 81
245	53. 88	273	80. 98	301	112. 98
246	54. 68	274	82. 01	302	114. 15
247	55. 54	275	83. 13	303	115. 32
248	56. 42	276	84. 35	304	116. 50

TABLE CONTINUED.

Temperature. °	Force of Vap. in inches of Mercury.	Temperature. °	Force of Vap. in inches of Mercury:	Temperature: °	Force of Vap. in inches of Mercury.
305	117. 68	312	125. 85	319	133. 86
306	118. 86	313	127. 00	320	135. 00
307	120. 03	314	128. 15	321	136. 14
308	121. 20	315	129. 29	322	137. 28
309	122, 37	316	130. 43	323	138. 42
310	123. 53	317	131. 57	324	139. 56
311	124. 69	318	132. 72	325	140. 70

On Vapour from Ether, &c.

We come now to the consideration of vapour from other liquids. Some liquids are known to be more evaporable than water; as, liquid ammonia, ether, spirit of wine, &c. others less; as, quicksilver, sulphuric acid, liquid muriate of lime, solution of potash, &c. and it appears that the force of vapour from each in a vacuum is proportionate to its evaporability. M. Betancourt maintains that the force of vapour from spirit of wine is in a constant ratio to that from water at all temperatures; namely, as 7 to 3 nearly. My first experiments with spirits of wine led me to adopt this conclusion, and naturally suggested that the force of vapour from any other liquid would bear a constant ratio to that of water. The principle however is not true, either with regard to spirit of wine or any other liquid. Experiments made upon six different liquids agree in establishing this as a general law; namely, *that the variation of the force of vapour from all liquids is the same for the same variation of temperature, reckoning from vapour of any given force:* thus, assuming a force equal to 30 inches of mercury as the standard, it being the force of vapour from any liquid boiling in the open air, we find *aqueous* vapour loses half its force by a diminution of 30° of temperature; so does the vapour of any other liquid lose half its force by diminishing its temperature 30° below that in which it boils; and the like for any other increment or decrement of heat. This being the case, it becomes unnecessary to give distinct tables of the force of vapour from different liquids, as one and the same table is sufficient for all.—But it will be proper to relate the experiments on which this conclusion rests.

Experiments on Sulphuric Ether.

The ether I used boiled in the open air at 102°.—I filled a barometer tube with mercury, moistened by agitation in ether. After a few minutes a portion of ether rose to the top of the mercurial column, and the height of the column became stationary. When the whole had acquired the temperature of the air in the room, 62°, the mercury stood at 17. 00 inches, the barometer at the same time being 29. 75. Hence the force of vapour from ether at 62° is equal to 12. 75 inches of mercury, which accords with the force of aqueous vapour at 172°, temperatures which are 40° from the respective boiling points of the liquids. By subsequent observations I found the forces of the vapour from ether in all the different temperatures from 32° to 102° exactly corresponded with the forces of aqueous vapour of the like range, namely from 142° to 212°: the vapour from ether depresses the mercury about 6 inches in the temperature of 32°.

Finding that ether *below* the point of ebullition agreed with water below the said point, I naturally concluded that ether *above* the point would give the same force of vapour as water above it; and in this I was not disappointed; for, upon trial it appeared that what I had inferred only from analogical reasoning respecting the force of aqueous vapour above the boiling point, actually happened with that from ether above the said point. And ether is a much better subject for experiment in this case than water, because it does not require so high a temperature.

I took a barometer tube of 45 inches in length, and having sealed it hermetically at one end, bent it into a syphon shape, making the legs parallel, the one that was close being 9 inches long, and the other 36. Then conveyed two or three drops of ether to the end of the closed leg, and filled the rest of the tube with mercury, except about 10 inches at the open end. This done, I immersed the whole of the short leg containing the ether into a tall glass containing hot water; the ether thus exposed to a heat above the temperature at which it boils, produced a vapour more powerful than the atmosphere, so as to overcome its pressure and raise a column of mercury besides, of greater or less length according to the temperature of the water. When the water was at 147° the vapour raised a column of 35 inches of mercury, when the atmospheric pressure was 29. 75: so that vapour from ether of 147° is equivalent to a pressure of 64. 75 inches of mercury; agreeing with the force of aqueous vapour of 257°, according to the preceding estimation: in both cases the temperatures are 45° above the respective points of ebullition. In all the temperatures betwixt 102 and 147° the forces of ethereal vapour corresponded with those of aqueous vapour, as per table, betwixt 212° and 257°. I could not reasonably doubt of the equality continuing in higher temperatures; bnt the force increases so fast with the increase of heat, that one cannot extend the experiments much farther without tubes of very inconvenient lengths. Being desirous however to determine the force of the ethereal vapour experimentally up as high as 212°, I contrived to effect it as follows:—Took a syphon tube such as described above, only not quite so long, and filled it in the manner above mentioned, with ether and mercury, leaving about ten inches at the top of the tube vacant; then having graduated that part into equal portions of capacity, and dried it from ether, I drew out the end of the tube to a capillary bore, cooled it again so as to suffer the internal atmospheric air to be of the proper density, and suddenly sealed the tube hermetically, thus inclosing air of a known force in the graduated portion of the tube. Then, putting that part of the tube containing ether into boiling water, vapour was formed which forced the mercurial column upwards and condensed the confined air, till at length an equilibrium took place. In this way I found 8. 25 parts of atmospheric air of the force 29. 5 were condensed into 2. 00, at the same time a perpendicular column of 16 inches of mercury in addition pressed upon the vapour. Now the force of elastic fluids being inversely as the space, we have 2. 00 : 29. 5 :: 8. 25 : 121. 67 inches = the force of the air within; to which adding 16 inches, we obtain 137. 67 = the whole force sustained by the vapour, measured in inches of mercury. The force of aqueous vapour, at the same distance beyond the boiling point, or 322°, is equal to 137. 28, per table. Thus it appears that in every part of the scale on which experiments have been made, the same law of force is observable with the vapour of ether as of water.

Experiments on Spirit of Wine.

By boiling a small portion of the spirit I used (about one cubic inch) in a phial, the thermometer stood at 179° at the commencement; but by continuing the ebullition it acquired a greater heat. The reason is, the most evaporable part of the spirit flies off during the process of heating, and the rest being a weaker compound, requires a stronger heat. The true point of ebullition, I believe, was nearly 175°.—The force of the vapour from this spirit at the temperature of 212°, I found both by an open syphon tube and one hermetically sealed with atmospheric air upon the mercurial column, as with ether, to be equal to 58½ inches of mercury. This rather exceeds the force of aqueous vapour at an equal distance from the boiling point; but it is no more than may be attributed to unavoidable little errors in such experiments. In a barometer tube the spirituous vapour at 60°, over the mercury, depresses the column about 1. 4 or 1. 5 inches; which is something less than the due proportion; one cause of this may be the evaporability of spirits, which in operating on small quantities, quickly dissipates part of their strength.

Experiments on Liquid Ammonia.

Liquid ammonia or volatile alkali, the specific gravity of which was .9474, boiled near 140°; in the barometer a small quantity depressed the mercury 4. 3 inches in the temperature of 60°. In higher temperatures it did not produce a proportional depression; because the most volatile part of the compound, expanding in the vacuum of the barometer, leaves the rest more watery, and consequently its vapour must be weaker; especially when the portion used is confined to a drop or two.

Muriate of Lime.

Put a portion of liquid muriate of lime over the column of mercury in a barometer. The boiling point of the muriate was found by experiment to be 230°. At 55° the depression was ,22 of an inch:

at 65°—.30
— 70°—.40
— 95°—.90

all which nearly agree with the forces of aqueous vapour 18° below the respective temperatures.

Mercury and Sulphuric Acid.

Mercury boils by my thermometer at 660°, and sulphuric acid of the specific gravity 1. 83, boils at 590°. It is very difficult to determine the precise force of vapour from these liquids in any temperature under 212°; because at such great distance from the boiling point the vapour is so weak as to be in effect almost imperceptible. Following the general law, the vapours of these fluids ought to be of the force .1, mercury at 460°, and sulphuric acid at 390°.—Col. Roi makes the expansion of 30 inches mercury by 180° of heat = .5969 or .5651; and in a barometer the expansion in the same circumstances is .5117; the differences are .0852 and .0534 which should measure the effective force of mercurial vapour of 212°, nearly. This is in all probability too much; as it is next to impossible to free any liquid entirely from air; and if *any* air enter the vacuum, it unites its force to that of the mercurial vapour.

That the force of vapour from sulphuric acid, in low temperatures, is exceedingly small, will appear from the ensuing section.

SECTION II.

On Vapour in Air.

The experiments under this head were made with manometers, or straight tubes of different lengths, hermetically sealed at one end, of $\frac{1}{15}$ inch internal diameter, and their capacities divided into equal portions. A drop or two of the liquid, the subject of experiment, was conveyed to the bottom or sealed end of the tube; the internal surface was then dried by a wire and thread, and atmospheric, (or any other air) was admitted into the tube, upon which a column of mercury was suspended of $\frac{1}{10}$ of an inch, or of 30 inches, less or more, according to the nature of the experiment. By immersing the end of the manometer, containing the air thus circumstanced, into a tall glass vessel containing water of any temperature, the effect of the vapour in expanding the air could be perceived. It was first indeed necessary to determine the increase air unaffected by any liquid (except mercury) would obtain by increase of temperature: that was done, as will be particularly shewn in the next essay. The expansion of all elastic fluids, it seems probable, is alike or nearly so, in like circumstances; 1000 parts of any elastic fluid expands nearly in a uniform manner into 1370 or 1380 parts by 180° of heat.

It will be unnecessary to repeat in detail the numerous experiments made on the various liquids in all temperatures from 32° to 212°; as the results of all agree in one general rule or principle, which is this: let 1 represent the space occupied by any kind of air of a given temperature and free from moisture; $p =$ the given pressure upon it, in inches of mercury; $f =$ the force of vapour from any liquid in that temperature, in vacuo; then, the liquid being admitted to the air, an expansion ensues, and the space occupied by the air becomes immediately, or in a short time $= 1 + \frac{f}{p-f}$; or which is the same thing, $= \frac{p}{p-f}$.

Thus in water for instance:

Let $p = 30$ inches,

$f = 15$ inches, to the given temp. 180°.

Then, $\frac{p}{p-f} = \frac{30}{30-15} = 2$, for the space; or the air becomes of twice the bulk.

If the temperature be 203°, $f = 25$, and the space becomes 6 times as large as at first.

If $p = 60$ inches

$f = 30$ inches to the given temperature

212°; then the space $= \frac{60}{60-30} = 2$; or water under the pressure of 60 inches of mercury, and at the temperature of 212°, produces vapour which just doubles the volume of air.

If ether be the instance: let the temperature be equal 70°; then $f = 15$; and suppose $p = 30$; in this case the colume of air is doubled; that is, ether of 70° being admitted to any portion of air, doubles its bulk.

The expansion of hydrogenous gas and atmospheric air by the vapour of water is the same for every temperature.

Sulphuric acid does not expand atmospheric air to any sensible amount by the heat of boiling water.

The theory of these facts is evident upon the principles laid down in the former essay: for instance; let it be required to explain the experiment with water of 212° under a pressure of 60 inches. Here the air was condensed into the space 1 by the pressure of 60 inches; but being exposed to water of 212° a vapour arose from it equal in force to 30 inches; the air therefore expanded till its force also became = to 30 inches, which was effected by doubling its volume: then the vapour pressing with 30 inches force and the air also with 30 inches force, the two together support the pressure of 60 inches and the equilibrium continues.—In short, in all cases the vapour arises to a certain force, according to temperature, and the air adjusts the equilibrium, by expanding or contracting as may be required.

The notion of a chemical affinity subsisting between the gases and vapours of different kinds, cannot at all be reconciled to these phenomena. To suppose that all the different gases have the same affinity for water might indeed be admitted if we could not explain the phenomena without it; but to go further, and suppose that water combines with every gas to the same amount as its vapour in vacuo; or in other words, that the elasticity of the compound should be exactly the same as if the two were separate, is certainly going far to serve an hypothesis.

Besides, we must on this ground suppose that all the gases have the same force of affinity for any given vapour; a supposition that cannot be admitted as having any analogy to other established laws of chemical affinity.

ESSAY III.

On Evaporation.

When a liquid is exposed to the air, it becomes gradually dissipated in it: the process by which this effect is produced, we call *evaporation*.

Many philosophers concur in the theory of chemical solution: atmospheric air, it is said, has an affinity for water; it is a menstruum in which water is soluble to a certain degree. It is allowed notwithstanding by all, that each liquid is convertible into an elastic vapour in vacuo, which can subsist independently in any temperature; but as the utmost forces of these vapours are inferior to the pressure of the atmosphere in ordinary temperatures, they are supposed to be incapable of existing in it in the same way as they do in a torricellian vacuum: hence the notion of affinity is induced.—According to this theory of evaporation, atmospheric air (and every other species of air for aught that appears) dissolves water, alkohol, ether, acids, and even metals. Water below 212° is chemically combined with the gases; above 212° it assumes a new form, and becomes a distinct elastic fluid, called *steam:* whether water first chemically combined with air, and then heated above 212°, is detached from the air or remains with it, the advocates of the theory have not determined.—This theory has always been considered as complex and attended with difficulties; so much that M. Pictet and others have rejected it, and adopted that which admits of distinct elastic vapours in the atmosphere at all temperatures, uncombined with either of the principal constituent gases; as being much more simple and easy of explication than the other; though they do not remove the grand objection to it, arising from atmospheric pressure. It has however been made to appear in these essays, I presume, that the objection to it from pressure, is itself founded upon an ungrounded hypothesis.

Leaving the theory of evaporation for the present, we shall proceed to the experiments.

The following positions have been established by others, and need therefore only to be mentioned here.

1. Some fluids evaporate much more quickly than others.

2. The quantity evaporated is in direct proportion to the surface exposed, all other circumstances alike.

3. An increase of temperature in the liquid is attended with an increase of evaporation, not directly proportionable.

4. Evaporation is greater where there is a stream of air than where the air is stagnant.

5. Evaporation from water is greater the less

the humidity previously existing in the atmosphere, all other circumstances the same.

The objects in view in this essay, are,

1. To determine the precise effect that a variation of temperature has upon the quantity evaporated.

2. To determine the ratio of evaporability of different fluids.

3. To find a rule by which the quantity and effect of previous humidity in the air may be ascertained.

4. From these and other facts to obtain a true theory of evaporation.

On the Evaporation of Water at 212°.

I took a small cylindrical vessel of tin, its diameter 3¼ and depth 2½ inches; and having fixed three pieces of wire to equidistant points of the circumference, they were fastened together at the top and the extremities bent into a hook, by which the vessel might be suspended from the end of a balance, &c. This done, the vessel was nearly filled with water, which was then made to boil over a small red fire in different circumstances: it was held in the hand and removed nearer to or further from the fire, so as to be kept just at the point of ebullition. In this state the vessel and water were weighed true to a grain, and the instant of time noted by a watch; then kept as above at 212° for ten minutes or more and again weighed: and the loss of water by evaporation, per minute, was thus ascertained. The experiments were repeated several times in the same as well as in different circumstances; and the results in no instance differed materially when obtained in the same circumstances.

The least evaporation per minute was 30 grains: this was when the fire, or lamp, was in the middle of a room, the doors and windows shut, and the air calm.

The next degree was 35 grains per minute or thereabouts: this was when the evaporating vessel was over a small fire in the usual fire-place; there being a moderate draught of air, and the room close.

A brisker fire, causing a stronger current of air up the chimney, gave from 35 to 40 grains per minute.

When the windows of the room were open, and a strong wind prevailed, the draught over the fire was proportionally increased, and the evaporation was from 40 to 45 grains per minute.

The extremes that have thus been noticed are 30 and 45 grains per minute: but were the experiment tried in the open air in high winds, I am inclined to believe from a comparison of the observations, that an evaporation of 50, 55 or even 60 grains per minute might be observed.

On the Evaporation of Water below 212°.

I have frequently tried the evaporation at all the temperatures below 212°: it would be tedious to enter into detail of all the experiments, but shall give the results at some remarkable points. In all the high temperatures I used the vessel above mentioned, keeping a thermometer in it, by which I could secure a constant heat, or at least keep it oscillating within narrow limits.

The evaporation from water of 180° was from 18 to 22 grains per minute, according to circumstances; or about ½ of that at 212°.

At 164° it was about ⅓ of the quantity at the boiling temperature; or from 10 to 16 grains per minute.

At 152° it was only ¼ of that at boiling; or from 8 to 12 grains, according to circumstances.

The temperature of 144° afford $\frac{1}{5}$ of the effect at boiling; 138° gave $\frac{1}{6}$; &c.

Having previously to these experiments determined the force of aqueous vapour at all the temperatures under 212°, I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the same temperature, and was agreeably surprised to find that they exactly corresponded in every part of the thermometric scale: thus the forces of vapour at 212°, 180°, 164°, 152°, 144° and 138° are equal to 30, 15, 10, 7½, 6 and 5 inches of mercury respectively, and the grains of water evaporated per minute in those temperatures were 30, 15, 10, 7½, 6 and 5 also; or numbers proportional to these. Indeed it should be so from the established law of mechanics, that all effects are proportional to the causes producing them. The atmosphere, it should seem, obstructs the diffusion of vapour, which would otherwise be almost instantaneous, as in vacuo; but this obstruction is overcome in proportion to the force of the vapour. The obstruction however cannot arise from the *weight* of the atmosphere, as has till now been supposed; for then it would effectually prevent any vapour from arising under 212°: but it is caused by the *vis inertiæ* of the particles of air; and is similar to that which a stream of water meets with in descending amongst pebbles.

The theory of evaporation being thus manifested from experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures, regard must be had to the force of vapour actually existing in the atmosphere at the time. For instance, if water of 59° were the subject, the force of vapour of that temperature is $\frac{1}{60}$ of the force at 212°, and one might expect the quantity of evaporation $\frac{1}{60}$ also; but if it should happen, as it sometimes does in summer, that an aqueous atmosphere to that amount does already exist, the evaporation, instead of being $\frac{1}{60}$ of that from boiling water, would be nothing at all. On the other hand, if the aqueous atmosphere were less than that, suppose $\frac{1}{2}$ of it, corresponding to 39° of heat, then the effective evaporating force would be $\frac{1}{120}$ of that from boiling water; in short, the evaporating force must be universally equal to that of the temperature of the water, diminished by that already existing in the atmosphere. In order to find the force of the aqueous atmosphere I usually take a tall cylindrical glass jar, dry on the outside, and fill it with cold spring water fresh from the well; if dew be immediately formed on the outside, I pour the water out, let it stand a while to increase in heat, dry the outside of the glass well with a linen cloth, and then pour the water in again; this operation is to be continued till dew ceases to be formed, and then the temperature of the water must be observed; and opposite to it in the table (page 559) will be found the force of vapour in the atmosphere. This must be done in the open air, or at a window; because the air within is generally more humid than that without. Spring water is generally about 50°, and will mostly answer the purpose the three hottest months in the year: in other seasons an artificial cold mixture is required.—The accuracy of the result obtained this way I think scarcely needs to be insisted upon. Glass, and all other hard, smooth substances I have tried, when cooled to a degree below what the surrounding aqueous vapour can support, cause it to be condensed on their surfaces into water. The degree of cold is usually from 1 to 10 below the mean heat of the 24 hours; in summer I have often observed the point as high as 58° or 59°, corresponding to ½ an inch of mercury in force, and once or twice have seen it at 62°: in changeable and windy weather it is liable to considerable fluctuation; but this is not the place to enlarge upon it.

For the purpose of observing the evaporation in atmospheric temperatures I got two light tin vessels, the one 6 inches in diameter and ½ inch deep, the other 8 inches diameter and ¼ inch deep; and made to be suspended from a balance, like the former one. When any experiment designed as a test of the theory was made, a quantity of water was put into one of these (generally the 6 inch one, which I preferred) the whole was weighed to a grain; then it was placed in an open window or other exposed situation for 10 or 15 minutes, and again weighed to ascertain the loss by evaporation; at the same time the temperature of the water was observed, the force of the aqueous atmosphere ascertained as above, and the strength of the current of air noticed. From a great variety of experiments made both in the winter and summer, and when the evaporating force was strong and weak, I have found the results entirely conformable with the above theory. The same quantity is evaporated with the same evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the same evaporating force, a strong wind will double the effect produced in a still atmosphere. Thus, if the aqueous atmosphere be correspondent to 40° of temperature and the air be 60°, the evaporation is the same as if the aqueous atmosphere were at 60° of temperature and the air 72°; and in a calm air the evaporation from a vessel of 6 inches in diameter in such circumstances would be about .9 of a grain per minute, and about 1. 8 grains per minute in a very strong wind; the different intermediate quantities being regulated solely by the force of the wind.

The following table exhibits the ratios and quantity of water evaporated in each temperature, derived from the preceding theory, and confirmed by experiments, as far as they have been extended. The first column expresses the temperature; the second, the corresponding force of vapour taken from the preceding table; the other three columns give the number of grains of water that would be evaporated from a surface of 6 inches in diameter in the respective temperatures, on the supposition of there being previously no aqueous vapour in the atmosphere. These columns present the extremes and the mean of evaporation, likely to be noticed, or nearly such; for, the first is calculated upon the supposition of 35 grains loss per minute from the vessel of 3¼ inches in diameter; the second, 45 and the third 55 grains per minute.

TABLE

Shewing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expressed in grains of water that would be raised per minute from a vessel of six inches in diameter, supposing there were no vapour already in the atmosphere.

Temperature.	Force of Vap. inch.	Evaporating Force in Grains.		
212°	30	120	154	189
20°	.129	.52	.67	.82
21	.134	.54	.69	.85
22	.139	.56	.71	.88
23	.144	.58	.73	.91
24	.150	.60	.77	.94
25	.156	.62	.79	.97
26	.162	.65	.82	1. 02
27	.168	.67	.86	1. 05
28	.174	.70	.90	1. 10
29	.180	.72	.93	1. 13
30	.186	.74	.95	1. 17
31	.193	.77	.99	1. 21
32	.200	.80	1. 03	1. 26
33	.207	.83	1. 07	1. 30

TABLE CONTINUED.

Temperature.	Force of Vap. inch.	Evaporating Force in Grains.		
212°	30	120	154	189
34°	.214	.86	1. 11	1. 35
35	.221	.80	1. 14	1. 39
36	.229	.92	1. 18	1. 45
37	.237	.95	1. 22	1. 49
38	.245	.98	1. 26	1. 54
39	.254	1. 02	1. 31	1. 60
40	.263	1. 05	1. 35	1. 65
41	.273	1. 09	1. 40	1. 71
42	.283	1. 13	1. 45	1. 78
43	.294	1. 18	1. 51	1. 85
44	.305	1. 22	1. 57	1. 92
45	.316	1. 26	1. 62	1. 99
46	.327	1. 31	1. 68	2. 06
47	.339	1. 36	1. 75	2. 13
48	.351	1. 40	1. 80	2. 20
49	.363	1. 45	1. 86	2. 28
50	.375	1. 50	1. 92	2. 36
51	.388	1. 55	1. 99	2. 44
52	.401	1. 60	2. 06	2. 51
53	.415	1. 66	2. 13	2. 61
54	.429	1. 71	2. 20	2. 69
55	.443	1. 77	2. 28	2. 78
56	.458	1. 83	2. 35	2. 88
57	.474	1. 90	2. 43	2. 98
58	.490	1. 96	2. 52	3. 08
59	.507	2. 03	2. 61	3. 19
60	.524	2. 10	2. 70	3. 30
61	.542	2. 17	2. 79	3. 41

TABLE CONTINUED.

Temperature. 212°	Force of Vap. inch. 30	Evaporating Force in Grains. 120	150	182
62°	.560	2. 24	2. 88	3. 52
63	.578	2. 31	2. 97	3. 63
64	.597	2. 39	3. 07	3. 76
65	.616	2. 46	3. 16	3. 87
66	.635	2. 54	3. 27	3. 99
67	.655	2. 62	3. 37	4. 12
68	.676	2. 70	3. 47	4. 24
69	.698	2. 79	3. 59	4. 38
70	.721	2. 88	3. 70	4. 53
71	.745	2. 98	3. 83	4. 68
72	.770	3. 08	3. 96	4. 84
73	.796	3. 18	4. 09	5. 00
74	.823	3. 29	4. 23	5. 17
75	.851	3. 40	4. 37	5. 34
76	.880	3. 52	4. 52	5. 53
77	.910	3. 65	4. 68	5. 72
78	.940	3. 76	4. 83	5. 91
79	.971	3. 88	4. 99	6. 10
80	1. 00	4. 00	5. 14	6. 29
81	1. 04	4. 16	5. 35	6. 54
82	1. 07	4. 28	5. 50	6. 73
83	1. 10	4. 40	5. 66	6. 91
84	1. 14	4. 56	5. 86	7. 17
85	1. 17	4. 68	6. 07	7. 46

The use of this table will appear from the following problems:

PROBLEM I.

Having given the temperature at which the aqueous atmosphere begins to be condensed into water, and the temperature of the air, to find the quantity of water that would be evaporated in a minute from a vessel of 6 inches diameter.

Solution. Subtract the grains opposite to the lower temperature from those opposite to the higher one, in the first, second or third column of grains, according to the strength of the wind, and the remainder will be the quantity evaporated in a minute, under those circumstances, nearly.

Example. Let the point of condensation be 52°, the temperature of the air 65°, with a moderate breeze.

The number opposite 52° in the second column of grains is 2. 06, and that opposite 65° is 3. 16; the difference, 1. 1 grain, is the evaporation per minute.

PROBLEM II.

Having given the quantity evaporated in a minute, found by experiment, and the temperature of the air, to find the force of the aqueous atmosphere, and the point of condensation.

Solution. Subtract the observed evaporation from that opposite the given temperature in the table; and look above for the number nearest to the remainder in the same column of evaporation, opposite to which will be found the force of the aqueous atmosphere, and the point at which it begins to be condensed.

Example. Finding the evaporation from a vessel of 6 inches in diameter to be 1. 7 grain per minute with a brisk wind, air 62°; what is the weight of the aqueous atmosphere, and the temperature at which it begins to be condensed into water?

The number opposite 62° in the third column of grains is 3. 52, being the whole evaporating force at that temperature in a perfectly dry atmosphere; from which take 1. 7 grains, the real evaporating force observed, and the remainder, 1. 82, corresponds, as per table, to the force .294 inches of mercury, the weight of vapour, and to 43° of temperature*.

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* It may be proper to remind the reader that all the experiments on evaporation are understood to be made in the open air, or in a window with a current inward; also it may be observed the evaporation in a close room is much less and is besides irregular, being greater proportionably from a less surface, evidently from the stagnation of the air.

Evaporation of Spirits, Ether, &c.

If the law of evaporation above given apply to water in every part of the scale of heat, no reasonable doubt can be entertained respecting its application to other liquids. I have notwithstanding made several experiments on others, the results of which are conformable to the same law. Some of them follow:—

1. Spirit of wine.—Evaporated from a surface of 4 inches in diameter, 54 grains in 25 minutes: air 53°; aqueous atmosphere at 49°, and beginning to rain with a moderate breeze. It would proportionally have been 121 grains from a vessel of 6 inches in diameter. This gives nearly 5 grains per minute. The same spirit boiled at or near 180°.

Now from the *data*, water of 83° is equivalent in force to spirits of 53°: and it may be seen that the evaporating force of water of 83° is nearly 5 in the first and second columns of grains of the table. It seems probable that the aqueous atmosphere does not diminish the evaporation of spirits as it does that of water.

2. Ether. 1. Put a phial containing ether, and a small tin vessel of 1¼ inch diameter into a scale and balanced them exactly: then poured the ether into the evaporating vessel and put

the phial into the scale again; took out 40 grains from the opposite scale, and waited till the equilibrium was restored: this was in 8 minutes 6 seconds. The air was 50°, and the ether at first 50°; but it rapidly sunk, as was found by dipping a very small bulbed thermometer into it, to 28°. In a window with a moderate breeze.

——— 2 and 3. Repeated the experiment in the same circumstances, except the evaporating vessel, which was now porcelain, and 2½ inches diameter. Lost 40 grains in 3 minutes. Thermometer sunk from 50 to 30°. The two experiments made this way did not differ above one or two grains.

These results reduced to a vessel of 3¼ inches in diameter give

1st. Experiment, loss 17 grains per minute;
2 & 3 ——— — 22½ ————

The reason why the result in the first experiment was something less than in the other two, was evidently owing to the circumstance of its longer duration, by which the ether was the greater part of the time in a low temperature, and consequently evaporated less.—The ether used boiled at 102°. At 50° it was therefore in the capacity of water at 160°. But water at 160°, at most loses only 17 or 18 grains per minute, and less

20° below that temperature. At first view therefore it should seem that ether evaporates quicker than the general law assigns.—But it must be allowed that the temperature of the *air* has some effect upon evaporation, though it has certainly very little. Now ether in the above experiments is acted upon by a current of air of an equal or higher temperature than itself; but water of 160° is usually acted upon by air 100° lower than itself, which is every moment precipitating the vapour formed, and thus obstructing its circulation. This appears to be a sufficient cause for the small difference observed.

With respect to mercury, sulphuric acid, muriate of lime, &c. there can be no doubt but they experience a real evaporation like those above; but it must be very small in proportion as their boiling points are high. And it would be difficult to make experiments upon such of these as have an affinity for aqueous vapour; because their acquisition from the aqueous atmosphere would far exceed their loss by evaporation.

Since writing the above essay, opportunities have occurred to ascertain whether the evaporation from ice is conformable to the same law as that from water. Every one, who has tried the experiment, admits the fact that ice is evaporable.—I have lately made several observations on this subject, the results of which, as far as they go, support the conclusion that the general law of evaporation continues the same below the point of congelation as above it. All the experiments were made in the tin vessel above described of 6 inches in diameter; a quantity of water was suffered to freeze in it, so as to form a circular cake of ice; the vessel and ice were then weighed together, and exposed in the open air for a certain time, after which being again weighed, the loss was found; the force of the aqueous atmosphere was sometimes determined during the experiment by a mixture of pounded ice and salt, in the manner already described.

	Grs.		H.		Grs.		Wind.	Air.
Nov. 5. In the night lost	110	in	9	; or,	.20	per m.	N. E. brisk.	28° to 31°
—— at 10 A. M. —	25	in	1¼	; or,	.33	——	N. E. mod.	32°
—29. at 1 P. M. —	24	in	1¾	; or,	.23	——	calm.	31°
P. M. —	84	in	9¼	; or,	.15	——	———	30°
—30. in the night —	94	in	9	; or,	.17	——	N. E. mod.	31°
Dec. 19. P. M. —	75	in	8	; or,	.16	——	N. E. calm.	26°—28°
In the night —	33	in	11	; or,	.05	——	calm.	29°
—20. A. M. —	21	in	2	; or,	.175	——	W. mod.	31°

Some of these being made in the night, and of long duration, neither the temperature of the air, nor the force of the aqueous atmosphere could be fairly determined: the second experiment was made under every favourable circumstance, and the aqueous atmosphere found at 22°. By problem 2, at page 588 it would have been determined at $21\frac{1}{2}°$, using the second column of grains in the table.*

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* On the subject of evaporation it may be considered as unpardonable not to advert to De Saussure's valuable Essays on Hygrometry.

That excellent philosopher determined, by a well conceived experiment, that dry air of the temperature of 64° or 66°, imbibed aqueous vapour so as to increase its elasticity $\frac{1}{57}$ of the atmospheric pressure; and that a cubic foot of such air required 11 or 12 grains of water to produce the effect. By the table above at page 560 it appears the force of vapour at 61° = .54 = $\frac{1}{54}$ of 29 .5 inches, nearly. It is probable this difference is occasioned in part at least by the want of perfect dryness in the air he operated upon, which caused the increase of elasticity to be less than otherwise.—It was, I think, unfortunate that he attached so much importance to and confidence in his hygrometer; and that he adopted the theory of chymical solution of water in air, contrary to the facts he discovered, which seemed more reconcileable to the notion of aqueous vapour being a distinct elastic fluid. Indeed he is forced to acknowledge in the 1st. chap, of his Essay on the Theory of Evaporation, that in the ordinary temperature of the atmosphere, aqueous vapour is formed in the first instance a distant elastic fluid, and *after it has been converted into an elastic fluid*, it is dissolved by the air; "Je crois qu'il ne la dissout que lorsque l'action du "feu l'a convertie en vapeur elastique." Now if it can for a moment exist independently under the pressure

ESSAY IV.

On the Expansion of Elastic Fluids by Heat.

The principal occasion of this essay is another on the same subject by Messrs. de Morveau and du Vernois in the first vol. of the Annales de Chimie. It appearing to them that the results of the experiments of De Luc, Col. Roi, de Saussure, Priestley, Vandermonde, Ber-

of the atmosphere, why may it not continue to exist in that state?

His table of the weight of aqueous vapour in a cubic foot of air at different degrees of the thermometer, being derived from experiments with his hygrometer, except the standard one of 66° (15° Reaumur), is far from accurate; and the inaccuracy increases with the distance from the standard, which, as has been observed, appears to be nearly correct: in the higher temperatures he makes the water dissolved too little, and in the lower temperatures too much.—He says (§ 93) that the lowest he has seen the hygrometer in the open air, is 40; and that it indicated a reduction of temperature in the air amounting to 34° .7 (78° of Fahr.) was necessary in order to deposit dew. This observation alone is sufficient to render his hygrometer suspected; for, few who have attended to the formation of dew will admit the probability of so large a reduction being necessary in any climate or season: I believe it rarely requires 40° reduction in temperature in any part of the world to produce the effect.

thollet and Monge did not sufficiently accord with each other; and that it would be of importance to determine not only the whole expansion of each gas from two distant points, such as the freezing and boiling, but likewise whether that expansion be uniform in every part of the scale, they instituted a set of experiments expressly for those purposes. The result of which was, that betwixt the temperatures of 32° and 212°, the whole expansion of one gas differs much from that of another, it being in one instance about $\frac{4}{10}$ of the original, and in others more than 12 times that expansion; and that the expansion is much more for a given number of degrees in the higher than in the lower part of the scale. These conclusions were so extremely discordant with and even contradictory to those of others, that I could not but suspect some great fallacy in them, and found it in reality to be the fact: I have no doubt it arose from the want of due care to keep the apparatus and materials free from moisture.

My method of experimenting on this subject is simple, and therefore less liable to error. A straight manometer tube, such as has been mentioned, is duly divided into equal portions of capacity; it is then dried by a wire and thread, and the open end inserted through a cork into a phial containing sulphuric acid, in order that the aqueous vapour may be drawn out of the tube; this is essential if we operate in temperatures lower than that of the atmosphere, otherwise not. For want of this attention, Col. Roi, in his valuable paper in the Philos. Trans. vol. 67, has been led into some erroneous conclusions.—A small column of dry mercury is then let down to a proper point in the manometer, and it is ready for experiment with common air.

It requires some address to fill the manometer with any other gas.—I succeeded best as follows: filled the tube with dry mercury; then pushed down a wire with thread, so that when the wire was got to the end of the tube, a thick covering of thread just entered the open end, and held the mercury like a cork, so that the tube could be inverted without losing the contents; then having a glass funnel with a perforated cork over the water apparatus, containing the gas, I slipped the manometer through the hole in the cork, and putting my hand into the water under the funnel, drew the wire out of the manometer, and with it the mercury; upon which the gas entered the manometer. For carbonic acid gas, I opened the sealed end of the manometer, drew it out to a capillary bore, and forced a stream of the gas through the tube; then putting my finger on the other end, sealed it again by a blow-pipe, and let down a small column of mercury to the proper point.

When the manometer was to be exposed to a heat of 212°, I used a Florence flask, with a long glass tube corked into it, in such sort that as much of the manometer as was necessary to be exposed to the temperature might be in the tube; then water at the bottom of the flask was made to boil violently, so that a constant stream of vapour issued out of the top of the glass tube, which was found to raise the thermometer to 212°. Small specks of white paint were put upon the divisions of the manometer together with numbers which were discernible through the containing tube. For lower temperatures a deep tin vessel containing hot water was used, in which the manometer was immersed, the water being well agitated previously to each observation.

From a great many experiments made in this way on common air, and likewise upon hydrogenous gas, oxygenous and nitrous gases, and carbonic acid gas, I can assert that the conclusions of De Luc, Roi, Saussure, Berthollet, &c. are nearly accurate throughout, and that those of de Morveau and du Vernois are extremely inaccurate in the higher temperatures.

I have repeatedly found that 1000 parts of common air of the temperature 55° and common pressure, expand to 1321 parts in the manometer; to which adding 4 parts for the corresponding expansion of glass, we have 325 parts increase upon 1000 from 55° to 212°; or for 157° of the thermometric scale. As for the expansion in the intermediate degrees, which Col. Roi's experiments shew to be a *slowly diminishing* one above the temperature of 57°, but which de Morveau's on the contrary shew to be a *rapidly increasing* one in the higher part of the scale; I am obliged to allow that Col. Roi is right, though it makes in some degree against an hypothesis I have formed relative to the subject; he has certainly however made the diminution too great from 72° downwards, owing to his not perceiving that he actually *destroyed* a portion of the elastic fluid he was operating upon (aqueous vapour) in reducing its temperature so low; if his air had been previously dried by sulphuric acid, &c. he would not have found so remarkable diminution below 72°. My experiments give for 77½° above 55°, 167 parts; for the next 77½° only 158 parts: and the expansion in every part of the scale seems to be a gradually diminishing one in ascending.

The results of several experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas and nitrous gas, which were all the kinds I tried, agreed with those on common air not only in the total expansion, but in the gradual diminution of it in ascending: the small differences observed never exceeded 6 or 8 parts on the whole 325; and differences to this amount will take place in common air, when not freed from aqueous vapour, which was the situation of all my factitious gases.

Upon the whole therefore I see no sufficient reason why we may not conclude, that *all elastic fluids under the same pressure expand equally by heat*—and that *for any given expansion of mercury, the corresponding expansion of air is proportionally something less, the higher the temperature.*

This remarkable fact that all elastic fluids expand the same quantity in the same circumstances, plainly shews that the expansion depends *solely* upon heat: whereas the expansion in solid and liquid bodies seems to depend upon an adjustment of the two opposite forces of heat and chemical affinity, the one a *constant* force in the same temperature, the other a *variable* one, according to the nature of the body; hence the unequal expansion of such bodies. It seems therefore that general laws respecting the absolute quantity and the nature of heat, are more likely to be derived from elastic fluids than from other substances.

In order to explain the manner in which elastic fluids expand by heat, let us assume an hypothesis that the repulsive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words to its temperature reckoned from the point of total privation: then, since the diameter of each particle's sphere of influence is as the cube root of the space occupied by the mass we shall have $\sqrt[3]{1000} : \sqrt[3]{1325}$ (10 : 11, nearly) : : the absolute quantity of heat in air of 55°: the absolute quantity in air of 212°. This gives the point of total privation of heat, or absolute cold, at 1547° below the point at which water freezes. Dr. Crawford (On Animal Heat, &c. page 267) deduces the said point by a method wholly different to be 1532°.—So near a coincidence is certainly more than fortuitous.

The only objection I see to this hypothesis is, that it necessarily requires the augmentation of elastic fluids for a given quantity of heat to be greater in the higher temperatures than in the lower, because the cubes of a series of numbers in arithmetical progression differ more the larger the numbers or roots: but it has just been shewn that in fact an augmentation of a contrary kind is observed. This refers us to the consideration whether the mercurial thermometer is an accurate measure of the increments of heat; if it be, the hypothesis fails; but if equal increments of heat cause a greater expansion in mercury in the higher than in the lower temperatures, and that in a small degree, the fact noticed above instead of being an objection will corroborate the hypothesis.—Dr. Crawford determines the expansions of mercury to be very nearly in proportion to the increments of heat: M. De Luc makes them to be less for a given quantity of heat in the lower than in the higher part of the scale; and in a ratio that agrees with this hypothesis. Now as every other liquid we are acquainted with is found to expand more in the higher than in the lower temperatures; analogy is in favour of the conclusions of De Luc, that mercury does the same.

EXPLANATION OF THE PLATE.

The annexed plate is intended to illustrate the author's conception of the constitution of the atmosphere. The different marks or characters of the particles of the gases are merely arbitrary, and intended for distinction; the simple atmospheres are given nearly on their real densities, and the particles are arranged at equal distances from each other. In the compound atmosphere the same arrangement is made of each kind of particles as in the simple; but the particles of different kinds do not arrange at regular distances from each other; because it is supposed they do not repel each other.

A REVIEW *of some* EXPERIMENTS, *which have been supposed to* DISPROVE *the* MATERIALITY *of* HEAT.

BY WILLIAM HENRY.

READ JUNE 5, 1801.

The following remarks, on the subject of heat, were written soon after the publication of Count Rumford's Inquiry concerning the Source of the Heat evolved by Friction; and of the interesting essays of Mr. Davy, which appeared in Dr. Beddoes's West Country Contributions. They were transmitted to Dr. Beddoes, for publication, about the close of the year 1799; but circumstances, with which I am unacquainted, have, I believe, induced the Doctor to decline the continuation of his periodical work. These circumstances, I deem it necessary to state; because, had the essay been written nearer the period of its publication, it would probably have assumed a very different form. At present, I have not leisure to review the subject, or to attempt any material alteration; and still less, to examine, whether I have been anticipated by any of the authors, whose essays have been published, during the two last years.

A Review of some Experiments, which have been supposed to disprove the Materiality of Heat.

It has long been a question among philosophers, whether the sensation of heat, and the class of phenomena arising from the same cause, be produced by a peculiar kind of matter, or by motion of the particles of bodies in general. The former of these opinions, though far from being universally admitted, is now most generally received; and the peculiar body, to which the phenomena of heat are referred, has been denominated by M. Lavoisier, caloric. Against the doctrine of the French school, some forcible arguments have lately been advanced by Count Rumford and by Mr. Davy, both of whom have adopted that theory respecting heat, which assigns, as its cause, a motion among the particles of bodies.

The method of reasoning, employed by Mr. Davy, in proving the immateriality of the cause of heat, is the *reductio ad absurdum*, i. e. the oppugned theory is assumed as true, together with its applications; and facts are adduced, directly contradictory of the assumed principles.

I shall take the liberty of offering a statement of the argument, rather different from that of Mr. Davy; though I trust without misrepresentation, or any material omission.

Let heat be considered as matter; and let it be granted, that the temperature of bodies depends on the presence of uncombined caloric. Now, if the temperature of a body be increased, the free caloric, occasioning that elevation, must proceed from one of two sources; either 1stly. it may be communicated by surrounding substances; or 2dly. it may proceed from an internal source, i. e. from a disengagement of what before existed in the body, latent or combined. But the temperature of bodies is uniformly increased by friction and percussion, and, necessarily, in one of the foregoing modes.

I. Mr. Davy found, by experiment, that a thin metallic plate was heated, by friction in the exhausted receiver of an air pump, even when the apparatus was insulated, from bodies capable of supplying caloric, by being placed on ice. This experiment he considers as demonstrating, that the evolved caloric could not be communicated by surrounding bodies.

To the inference deduced from this experiment, it may be objected, that the mode of insulation was by no means perfect. Admitting the vacuum, produced by the air pump, to have been complete, still the supply of caloric could not thus be entirely cut off; since it has been shewn by Count Rumford, that caloric passes even through a torricellian vacuum. If, therefore, friction produce in bodies some change, which enables them to attract caloric from surrounding substances, this attraction may be equally efficient in an exhausted receiver, as in one containing an atmosphere of mean density. It would be an interesting subject of experiment, to determine the influence of atmospheres of various densities, as conductors of caloric; for, since effects are proportionate to their causes, and it is ascertained that common air conducts caloric, better than it is conveyed through a vacuum, as 1000 is to 702, it may be expected that the ratio will hold in all intermediate degrees.

In Count Rumford's* masterly experiment, the metal, submitted to friction, was encompassed by water; and air was carefully excluded from the surfaces in motion. Yet the water became hot, and was kept boiling a considerable time. In this case, the only obvious source of caloric, from without, was through the borer, employed in producing the friction; if it be true, as the Count has observed, that the water could not, at the same instant, be in the act of giving out and receiving heat. The same objection to the communication of heat, from an external source, exists, also, in thus explaining Mr. Davy's experiment: but I cannot admit that the argument is demonstrative, in proving the evolved caloric not to be derived from external substances; for no absurdity is implied in supposing, that a body may be receiving caloric in one state, and giving it out in another. We have an example of the simultaneous admission and extrication of a subtile fluid, the materiality of which is admitted by Mr. Davy, in an excited electric, which, at the very same instant, receives the electric fluid from without, and transfers it to the neighbouring conductors. In an ignited body, also, the two processes of absorption and irradiation of light, are, perhaps, taking place at the amse moment.

II. Another cause of the increase of temperature in bodies, is the liberation of their combined caloric; and, if this be a source of temperature, the absolute quantity of caloric in a body must be diminished by friction. That no such diminution really takes place, we have the evidence of two experiments—the one of Mr. Davy, the other of Count Rumford. Mr. Davy, by rubbing together two pieces of ice converted them into water. Now water, *ex hypothesi*, contains more caloric, than the ice, from which it was formed; and, on the same hypothesis, the absolute quantity of caloric in ice is diminished by friction and liquefaction, which is absurd. Count Rumford, also, ascertained that the specific heat of iron was not diminished, when converted by a borer into turnings, and consequently when it had been the source of much temperature. In explanation of these facts, we may be allowed to assume the communication of caloric from surrounding bodies, *till this* communication has been demonstrated to be impossible. But even were the impossibility established, it would yet remain to be proved, that the evolved caloric does not proceed from an internal source; and this can only be done, by an accurate comparison of the quantity of caloric in bodies, before and after friction. Now, in instituting this comparison, it is implied, that we possess means of determining the absolute quantity of caloric in bodies, and that we can compare quantities of caloric, with as much certainty, as we can obtain from an appreciation by weight or by measure. Such perfection, however, does not, I apprehend, belong to the present state of our knowledge respecting heat; for I have always been distrustful of that part of the doctrine, which assigns the ratio of heat latent in bodies. The grounds of this distrust I shall state pretty fully—for, if it can be proved that we have no accurate conceptions of quantity, as appertaining to heat, all arguments against its materiality, derived from supposed determinations of its quantity, must be inconclusive.

The only clear conceptions, which the mind has of quantity, are derived either from a comparison of the magnitude, or of the gravity, of bodies. In the instance of caloric, both these modes of mensuration fail us. We cannot estimate the bulk of a substance, which eludes our grasp and our vision; nor have we yet succeeded in comparing its gravity, with that of the grosser kinds of matter, which it surpasses in tenuity, beyond all comparison. Our notions of the quantity of caloric are derived, not from such simple judgments, but from complicated processes of reasoning, in the steps of which, errors, fatal to the whole, may, perhaps, sometime appear.

Whatever be the nature of caloric—whether it be a body *sui generis*, or a quality of other bodies,—its effects are peculiar and appropriate; and, like all other effects, bear a proportion to the energy of their cause. Expansion, for example, it is proved by experiment, keeps pace with the actual increments of heat; and on this principle is founded the thermometer, the great agent in the acquirement of all our ideas respecting heat, both absolute and relative. The competency of this instrument, however, to afford information of the quantity of caloric, is limited by the following circumstances.

1. The mercury of the thermometer indicates only the quantity of heat, which it has itself acquired, and by no means that contained in surrounding bodies. 2dly. The scale of expansion is wholly arbitrary, commencing far from the absolute privation of heat, and falling far short of its maximum. 3*dly*. The caloric, latent in bodies, or chemically combined with them, has no effect on the thermometer. 4thly. The experiments of Dr. Crawford, though sufficient to shew that the expansion of the mercury of the thermometer bears a ratio to the actual increments of heat, in any temperature between the boiling and freezing points of water, by no means prove that this proportion holds universally.

Equal weights of heterogeneous bodies, it is presumed, contain unequal quantities of caloric; and the ratio of these quantities, is approximated, in the following manner.

Equal weights of the same body, at different temperatures, give, on admixture, the arithmetical mean: but equal weights of different bodies, at different temperatures, afford a temperature, which varies considerably from the mean. Thus a pound of water at 100 degrees, and a pound at

200°, give the temperature of 150°; but a pound of water at 200° and a pound of mercury at 100° afford, not the mean, but a temperature considerably higher. Hence it follows that a pound of mercury has not the power of fixing and retaining so much caloric, as a pound of water: and the fixation of more heat, by the water than by the mercury, is ascribed to the superior energy of a power, inherent in both, and termed capacity for caloric.

From an extensive series of experiments Dr. Crawford infers, that the capacities of bodies are permanent, so long as they retain their form. Thus, the capacity of water has to that of mercury the ratio of 28 to 1, at any temperature between 32° and 212°. The difference of capacities of bodies, it is inferred, therefore, would continue the same, down to the absolute privation of temperature.—Imagine, then, two bodies, at this point of privation: they may still contain unequal quantities of combined caloric; for, when chemically combined, caloric does not produce temperature. On Dr. Crawford's hypothesis, these comparative quantities of combined caloric, in the two bodies, may be learned, by observing the ratio of temperature, produced, by the addition to each, of similar quantities of heat. This supposition, however, is manifestly gratuitous; and the contrary might be maintained, with equal or greater probability: for, it may be supposed, that at this assumed negation of temperature, one body renders latent more caloric than another, because it actually contains less; as certain dry salts attract more water from the atmosphere, than others containing much water of crystallization. The commonly employed mode of ascertaining the specific caloric of bodies, is founded, therefore, on an assumption, which is deficient in the character of a datum, and which itself requires proof.

If these objections be valid, they will apply also to shew the fallacy of the theorem, for finding the absolute zero of bodies. By this term some philosophers appear to understand the point of absolute privation of caloric, both free and combined. I apprehend, however, that in strict propriety it can only be used to signify the negation of *uncombined* caloric, or, as Dr. Crawford expresses himself, the point of absolute cold. As applied, however, to water, it is evident that the whole quantity of heat is understood.—In ascertaining the zero, say these calculators, the capacity of ice to that of water is as 9 to 10. It is plain, therefore, that when water freezes, it must give out $\frac{1}{10}$th. of its whole heat, and this 10th. part is found to answer to 146° of Faht. Consequently its whole heat is

10 times 146, or 1460°; and hence the natural zero is 1460 — 32 or 1428°. Now of this estimate it is a datum, that the capacities of ice and water have precisely the above ratio. But if the general formula, for ascertaining the specific caloric of bodies, be founded on erroneous principles, it cannot serve as the groundwork of any solid conclusions.

The materiality of caloric may, I apprehend, be maintained, without admitting that we have made any steps towards determining its quantity in bodies; and the arguments of Count Rumford and Mr. Davy are not demonstrative, because they assume, that this part of the doctrine of caloric cannot be relinquished, without abandoning it *in toto*. I may be permitted, therefore, to state my reasons for believing caloric to be matter; which would have been unnecessary, had the contrary been proved, with all the force of mathematical demonstration.

Avoiding all metaphysical reasoning on the nature of matter, and assuming the generally received definition, as sufficiently characterizing it, I shall examine how far this general character of matter applies to the individual—caloric. Caloric occupies space or is extended, because it enlarges the dimensions of other bodies; and, for the same reason, it is impenetrable, since if it could exist, at the same time, in the same place, with other bodies, their volume would never be enlarged by the addition of heat. Of form or figure, as only a mode of extension, it is unnecessary to prove that caloric is possessed; and indeed there is perhaps only one general quality of matter, that will not be allowed it, viz. attraction. That caloric is influenced by the attraction of gravitation, or by cohesive attraction, has never yet been proved. Yet the various experiments of Buffon, Whitehurst, Fordyce, Pictet, &c. cannot be alleged as proofs, that it is actually devoid of this property; since they only decide, that the small quantities, which can be artificially collected, are not to be set in the ballance against the grosser kinds of matter. One kind of attraction, *that* which has lately been termed chemical affinity, may, I think, after a full survey of phenomena, be fairly predicated of caloric—and if its possession of this quality be rendered probable, we shall thence derive a powerful argument, in favour of its materiality.

That chemical affinity has a considerable share in producing the phenomena of heat, appears probable from the following considerations.

1. All the characters, distinguishing caloric when separate, cease to be apparent, when it has contributed to a change of form in other

bodies; and the properties of the substances so changed are also materially altered. Now this is the only unequivocal mark of chemical union, that we can apply in any instance; and chemical union implies the existence and efficiency of chemical affinity.

2. The relation of caloric to different substances appears to observe that peculiar law, which, in other instances, is termed elective affinity. If a compound of two or more principles, a metallic oxyd for instance, be exposed in a high temperature, the caloric forms a permanent union with the one, but not with the other. In certain instances, caloric is evolved, when two substances, attracting each other more powerfully than they attract caloric, produce on admixture, an elevation of temperature. In other instances, caloric is absorbed, when it is attracted by the new compound, more strongly than by the separate components. Such facts warrant the deduction, that caloric is subject to the laws of chemical affinity.—But the precise order of its affinities remain to be decided, by future experiments.

3. Caloric seems, also, on some occasions, to bear a part in the operation of double elective affinities. In this way, it produces decompositions, which, by single affinity, it is incapable

of effecting.—Thus a most intense fire does not expel, entirely, the carbonic acid from alkalis. But when the affinity of an acid for an alkali concurs with that of carbonic acid for caloric, a decomposition ensues.—Again—Water may be submitted to the highest temperature, without imparting a gaseous form to the hydrogen which it contains; but the conspiring affinity of a metal for oxygen occasions the production of hydrogenous gas. On this principle, many chemical facts are resolved into the law of *double affinity*, which are, at present, explained by that of single elective attraction.

4. Caloric acts, sometimes, as an intermedium in combining bodies, which, without its aid, are not susceptible of combination. Thus carbon and oxygen do not evince any tendency to combination, at the ordinary temperature of the atmosphere; but caloric brings them into union, and constitutes, itself, part of the resulting compound. This, and a variety of other instances, have a striking resemblance to what is called *intermediate affinity*.

In the theory of Dr. Crawford, no influence is allowed to chemical affinity over the phenomena of heat; and indeed *that* philosopher expresses a decided opinion, that elementary heat is not capable of uniting chemically with bodies. Hence it appears, that the difference between the terms affinity and capacity is not merely a verbal one; but that they are actually expressive of different powers or causes: and the question, therefore, which of these terms shall be adopted, in the description of facts, is one involving the determination of causes.

The term capacity for heat is employed, by Dr. Crawford and others, to denote, in the abstract, that power, by which different kinds of matter acquire different quantities of caloric. But in the various applications, that are made of this theory, a more precise meaning is often affixed to it; and the term is applied, in much the same sense, which it has in common language. When thus understood, a difference of capacity necessarily implies a difference in the extent of the spaces, between the minute particles of bodies; and that these differences occasion the varieties, observed in the acquirement of heat by different bodies. On this theory, there is no active principle or power inherent in bodies, and more active in some than in others,—no tendency in the matter of heat to attach itself, in preference, to any one substance. The assigned cause of the phenomena of heat is not, I apprehend, adequate to produce the effects ascribed to it.

On the theory of capacities, a change of form is, in certain instances, antecedent to the absorption of caloric. Thus, when ether is converted into gas, on removing the pressure of the atmosphere, according to this hypothesis, the capacity of the ether is increased by its volatilization; and the change of form is prior to, and the cause of, the absorption of caloric. The order of events, then, in the volatilization of ether, is first an alteration of form; next a change of capacity; and lastly an absorption of caloric. On this hypothesis, ether may exist in the state of gas, without containing a greater absolute quantity of caloric, than in a liquid form. But such an interpretation of phenomena is directly contradictory to an established principle, admitted, even by those who prefer the doctrine of capacities, viz. that all bodies, *during* their conversion from a fluid to a vaporous state, absorb caloric. It is at variance, also, with observed facts: for if a thermometer be immersed in a portion of ether, confined under the receiver of an air pump, the temperature of the ether will be found to sink gradually, during the exhaustion of the air; and the evaporation becomes proportionally slower, till, at last, it is scarcely perceptible. We may, therefore, infer, that at a certain point of diminished temperature, the volatilization of ether would entirely cease, if the supply of caloric, from surrounding bodies, could be completely intercepted. But on the theory of capacities, the evaporation should proceed as rapidly at the close, as at the commencement, of the process—or, in other words, evaporation should be wholly independent of temperature, which every one knows is contrary to fact.

It may be considered, therefore, as extremely probable, that the tendency of ether to assume a gaseous form depends on its chemical affinity for caloric. But, (it may be asked) how is this affinity counteracted by an increased pressure, and augmented by a diminished one?

A circumstance, absolutely essential to the formation of gasses, is, that free space shall be allowed for their expansion.—Mechanical pressure acts as a counteracting force to this expansion; and either prevents it completely, or partially, according to the degree of its application. But from this fact, no argument can be drawn against the existence of chemical affinity, as an attribute of caloric. Two opposite forces in physics may be so balanced, that neither shall produce its appropriate effect. Thus a body, impelled in contrary directions, may remain at rest, yet the operation of the opposing forces, in this case, cannot be denied. Even in chemistry, we have unequivocal examples, in which the action of the affinities is suppressed by more powerful causes. Thus bodies, that have a strong chemical affinity, are kept perfectly distinct, even when placed in contact, by the affinity of aggregation. The only inference, then, that can fairly be deduced from the effects of pressure, in preventing the formation of gasses, is, that it is a power, sometimes superior, in energy, to that of chemical affinity.

Since, therefore, caloric is characterized by all the properties, except gravity, that enter into the definition of matter, we may venture to consider it as a distinct and peculiar body. Nor is its deficiency of gravity sufficient to exclude it from the class of material substances. Such nicety of arrangement might, with equal propriety, lead us to deny the materiality of light, the gravity of which has never yet been proved: for, besides the experiments of Mr. Michell, which failed in ascertaining this property of light, we have several chemical facts tending to the same conclusion. Thus Mr. Cavendish, after firing a mixture of hydrogenous and oxygenous gases, in a close vessel, a process during which much light is always emitted, found not the smallest diminution of weight.

To have completed this defence of the material nature of heat, it would have been proper to have pointed out the circumstances, in which the phenomena of heat differ from the known and acknowledged phenomena of motion. At present, however, I have not leisure to pursue the subject at much length; and, though several points of disagreement would doubtless be found, I shall mention only one of the most marked and decisive.

Motion is an attribute of matter, independently of which it cannot possibly subsist. If therefore, the phenomena of heat can be shewn to take place, where matter is not present, we shall derive, from the fact, a conclusive argument against that theory of heat, which assigns motion as its cause. Now, in the experiment of Count Rumford, before alluded to, heat passed through a torricellian vacuum, in which, it need hardly be observed, nothing could be present to transport or propagate motion. This experiment, in my opinion, decidedly proves, that heat can subsist independently of other matter, and consequently of motion— in other words *that heat is a distinct and peculiar body.*

An INVESTIGATION *of the* METHOD *whereby Men judge, by the Ear, of the* POSITION *of* SONOROUS BODIES *relative to their own Persons.*

BY MR. JOHN GOUGH.

COMMUNICATED BY DR. HOLME.

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The power of the ear to distinguish very slight variations of tone, has been observed long ago; and some experiments have been made to ascertain the degree of discrimination, which a good ear possesses in this respect. But there is *another* faculty of the auditory organs, which, as far as I know, has never been explained,—I mean the power of ascertaining, with some degree of precision, the bearings or relative positions of sonorous bodies. Every man is sensible, by constant experience and observation, of the existence of the faculty in question; for every person who can hear, knows, without the use of his eyes, from what quarter any particular sound proceeds; or, to speak in more definite terms, perceives, whether it comes from before or behind him, from his right hand or his left. His powers of discrimination in this respect are not confined to the general limits or points, which I have here mentioned by way of illustration; for, he can in most instances divide these principal angles, not indeed into degrees and minutes, but with so much accuracy as to know whether the sounding object lies in a line making a less angle with that imaginary right line, which may be supposed to join his ears; or with another right line, which, passing from the front to the back part of his head, bisects the former at right angles. This sort of perception is not confined to what passes upon the horizon; for, place the same person on a tower, or other steep eminence, and he will perceive whether a certain sound comes from a part below or above him: besides which, he immediately combines this sensation with the preceding, so as to form a judgment respecting the true situation of the sounding body, which is of great practical use in the common affairs of life.

The nature of the present essay seems to require, that the reader should conceive the human head to be divided by two fixed mathematical planes, with a view to assist his imagination while he peruses the sequel.

The first of these planes lies parallel to the horizon, when the hearer stands in an erect posture; and contains in it the imaginary right line that joins the ears, which in future will be called, the *axis of hearing*. This plane, therefore, divides the head into two dissimilar solids, the one superior, and the other inferior in respect to itself. The other plane must be conceived to stand perpendicular to the last, and to bisect the axis of hearing at right angles. On these accounts, it evidently passes from the front to the back part of the head, dividing it vertically into two portions; which are so nearly alike in most men, that they may be considered, without danger of error, as two equal and similar geometrical solids. The preceding description, which bears the face of a mathematical construction, may perhaps assist my readers in conceiving what was meant above, when I spoke of combining the perception produced by the comparative elevation or depression of a sounding body, with the sensation which arises from its place on the horizon, relative to the axis of hearing: for every man judges, with some degree of accuracy, of the angles which are made with the two planes described above, as well as their common section, by the right line that joins his head and the place from which any particular sound proceeds.

Though the fact I have been describing is established by universal experience, the cause of it, I believe, has never been investigated; and indeed the question, when we first attend to it, seems to put on a very puzzling appearance. We know that when sound finds a free passage through the air, it takes the shortest path, leading from its source to the person who attends to it; and that the sensation of hearing is occasioned by a succession of waves or pulses of air, which fall upon the auditory organs, in the direction of a right line drawn from the sounding body to the head of the hearer. But after the enquirer has arrived at this point in his examination of the problem, his progress is hindered by the difficulty under consideration. The perplexity here alluded to arises from the obscurity of the principle, whereby men compute the angles, which a right line, drawn from the place of a sound to the head of a hearer, makes with the two planes described above, as well as their common section.—It is in vain to endeavour at an explanation of the phænomenon, by analogies borrowed from vision.—The spectator judges of the relative positions of visible objects, by knowing the situation which their images have on the retina, in respect to the axis of his eye;

but the person, who judges by the ear, has not the same advantage in measuring angles: for whatever may be the direction of a sound in the open air, as soon as it enters the auditory passage, it is compelled to follow the course of that duct, until it reaches the apparatus in which the sense of hearing resides.—In consequence of this restriction, all sounds whatever fall on the seat of sensation in the same direction; viz. in the ultimate direction of the auditory passage. The foregoing circumstance must, it should seem, unfit the ear for judging of the comparative positions of sounding bodies; because, if like effects follow like causes, we must conclude, that the ear is incapable of perceiving any angular variation, arising from the situations of sounding bodies in respect to itself: seeing the pulses, proceeding from any number of such bodies any how disposed, are forced, by the construction of the auditory organ, to strike the sensorium under a given angle; for, ultimately they all move parallel to a given right line.—The impossibility of explaining this problem by analogy, shews the necessity of examining the nature of hearing itself, in order to discover a proper way of investigating the difficulty.

The great sensibility of the ear is, in my judgment, the real cause of the phænomenon, which constitutes the topic of the present essay. But it will be proper to say something, in the first place, respecting the accuracy of this organ in distinguishing the difference of two sounds that are known to be nearly equal in force; because the truth of my opinion rests on this fundamental fact.—The want of a sure method of measuring the momentum of the air when agitated by a vibrating body, with the same certainty that the angles between rays of light are measured, appears to be the reason why the accuracy in question is so generally overlooked. But though it seems very difficult to give a general rule for measuring magnitudes of this description, the following experiment proves, in a very satisfactory manner, what a delicate faculty the sense of hearing is.—A bolt, driven by a spring against a fixed piece of metal, may be made to produce a succession of strokes of equal force; consequently the concussions given to the air, by any two of these strokes, will also be equal; and will therefore occasion like effects on the same ear, placed at equal distances from the spring, the state of the wind and weather being the same in both cases.—I caused an instrument of the preceding description to be struck repeatedly at the distance of 40 feet from my ear, care being taken to place it in the axis of hearing produced; after which, it was moved in the same right line sometimes two feet further from me, at other times two feet nearer to my person; and I could always distinguish distances thus varied. The range of the sound, or the distance at which it ceased to be audible, was 240 feet, or six times the interval made use of in the experiment. The sound which I employed was therefore of a moderate force, and perhaps the interval was a suitable one, being neither too great nor too little a part of the whole range. It appears then, that a good ear will discover a perceptible difference in the forces of two equal sounds; the one of which moves through one sixth part of its whole range, and the other through a space which differs from the distance of the former only the 120th. part of the range common to them both.—The foregoing instance affords a remarkable proof of the ear's accuracy in comparing slight variations in the momenta of sounds; and I have reason to believe that the delicacy of my organs, in this respect, surpasses the medium of sensibility; for, some ears which were tried in the same manner, did not perceive the effect in question, until the instrument had been moved four feet, or the 60th. part of my range. But either instance furnishes a proof sufficient for the present purpose, and shews the human ear to be a very delicate judge of comparative loudness.

The nice faculty of discriminating sounds, nearly equal in force amongst themselves, being established by the preceding experiment, it is to be applied in the next place to explain the phænomenon we are treating of. When we find that successive parts of the same sound preserve their force or loudness unaltered, we are taught by experience to conclude, that the sounding body also preserves its distance from us unchanged: on the contrary, when successive parts of the same sound grow stronger or weaker, we know with equal certainty, that the space interposed between our ears and the vibrating object is, for the most part, shortened and lengthened accordingly, by changing the place of the hearer or of the sounding body. Experience also teaches men to use the same nicety of perception, in ascertaining the positions of sonorous objects with respect to their own persons. In order to illustrate this question, it will be proper to begin with a simple example. Suppose then a sounding body, situated towards the front of the hearer, to lie in the horizontal plane, mentioned in the beginning of this essay, and to be placed on one side of the right line, which bisects the axis of hearing at right angles:—for instance, let it be to the right hand of it.

Moreover, we will take for granted, what will be afterwards proved; namely, that the pulses of air, proceeding from the vibrating surface, will strike the right ear, which is turned towards it, more forcibly than the left. Under these circumstances, two cotemporary currents of the same sound will strike the opposite sides of his head, at the same time, with unequal momenta: he is therefore left to form a judgment of the incidents; and the sensibility in question enables him to determine with certainty, which of his ears is most affected. A thousand similar cases have occurred in the course of his life, in which he has been convinced by the testimony of his eyes and hands, that the strongest impression is constantly made on that ear which is turned towards the sounding body; he therefore draws the same conclusion in those cases where he is not assisted by the evidence of sight and touch. In this manner every man acquires a practical rule, which, like all practical rules formed in infancy, becomes a mechanical action, and is therefore exercised by every body, while very few understand the nature of it. Some of my readers may suspect, that on the present theory, too much influence is ascribed to experience. But such a suspicion would in my opinion be ill founded; for, all the senses have their practical maxims, which they borrow mutually one from another. Opticians have demonstrated, that vision would prove but of little use, were not its natural imperfections corrected by touch: hearing also forms its judgment in one class of phænomena, from a kind of secondary knowledge afforded by the hands and eyes. Two sounds moving through unequal spaces may be equal in force, nay the remoter may prove the more powerful of the two: *loudness*, therefore, is not a sure indication of proximity; but every man has remarked, at an age when his memory was too weak to record incidents, that sounds lose more and more of their asperities the farther they move: every man, therefore, in his riper years, considers *roughness* to be a proof of proximity, and smoothness to be the criterion of remoteness. That this faculty is acquired, appears evident from the errors into which it leads the judgment under certain circumstances. I have known a person mistake the mellow tone, which is produced by rubbing the brim of a glass vessel with a wet finger, for the blast of a distant horn, though there was but the breadth of a table between him and the place of the sound, and almost every one falls into a similar deception the first time he hears the soft notes of an Æolian harp. If then the ear correct its imperfections in one instance, by means of information derived from the other senses, it will not fail to make use of the same aids as often as it can do so with advantage; it will therefore have recourse to this secondary species of knowledge, provided the pulses of air, proceeding from a sounding body placed not directly before or behind the hearer, can be proved to fall with a greater force on one of his ears than on the other. I am apprized that this supposition is not countenanced by the prevailing theory of the propagation of sounds: according to which, the pulses of air diverge from every point in the circumference of an obstacle towards all parts beyond that impediment. But this theory appears to be a mathematical conception rather than a fact; it is therefore better fitted to explain the outlines of hydrostatics, than to assist in the minuter parts of a physical enquiry. The laws and properties of a perfectly elastic fluid do not apply to the atmosphere without exception; and an occurrence, which is not unfrequent, seems to prove decisively, that the pulses of air which move parallel to the axis of hearing strike only one ear with effect, I mean that which opposes itself to their progress. If you happen to address yourself in an open area to a person who has the misfortune of being deaf on one side, having your mouth directed to his defective ear, he will perceive nothing, or at most but a very confused noise. This circumstance shews, that the pulses which pass immediately before and behind him, continue in their course without being deflected to the sound ear, which it screened by the interposition of the temples and face, from the action of those that strike the contrary side of the head. But the case would be otherwise, provided sounds diverged from all points indifferently; because on this supposition the ear could not be protected from their influence by its situation. Any person has it in his power to make the same experiment on himself; for let any one standing in the open air close one of his ears with moist paper, and cover the same side of his head with a folded napkin or cushion; if then a watch be held in the axis of hearing, at the distance of two or three inches from the napkin or cushion, he will not be made sensible of its presence by sound. Should the same experiment afterwards be repeated in a confined apartment, the person who makes it will perceive the clicking of the watch in the direction of his open ear, because the beats are reflected in their natural succession from the side of the room which is opposite to that ear. Opportunities sometime occur, that enable us to contemplate the same phænomenon on a larger scale. If a lofty and extensive building happen to intervene between a person in motion, and the place from which a loud noise proceeds, he no longer hears the sound in its proper direction, provided he passes very near the edifice, though at the same time he is convinced that the noise still continues, by hearing it reflected from another quarter. Let him, in the next place, advance in a right line perpendicular to the side of the building, and he will in a short time lose the echo, and recognize the original sound. Perhaps it may be imagined, that this change will not be observable before a right line, drawn from the head of the hearer to the sounding body, barely touches the top of the edifice: but this supposition, which is true when applied to the phænomena of shadows, cannot be relied on in the present instance. For as often as a sound meets with interruption, those pulses which pass nearest the obstructing body propagate themselves afresh into the air lying on the contrary side of the impediment, by means of the lateral communication of motion; on which account, an intercepted sound recurs at a less distance from the obstacle, than that which a geometrician would assign to the event, by drawing the right line specified above. By the way, we may observe, that the principle which Signor Venturi has lately denominated the lateral communication of motion to fluids, has so considerable a share in the various phænomena of sound, as to render geometrical speculations on the subject in a great measure useless. For instance, if a lofty obstacle interpose itself between a sounding body placed at the foot of it and a person standing at any distance on the opposite side, according to geometry, he will not be able to perceive any sound from it, because the pulses which should strike his ear with effect, will be intercepted in their progress by that obstacle; on the contrary, we are convinced by experience, that he will hear it, not indeed in the true direction, but as if it proceeded from some place elevated above the impediment. Sound, therefore, is propagated in a manner which neither coincides with the commonly received theory relative to the subject, nor with the phænomena of shadows; on the other hand, it seems to follow a law, that may be said to form a medium between the two.

The consequences of interrupting the pulses of sound, as well as the lateral communication of motion being now explained, the topic of the essay may be resumed. The annexed diagram will be found of use in explaining the phænomena which arise from the pulses of sound being obstructed by the hearers head, as they move in the horizontal plane passing through his ears, which case ought to be treated separately from the more complex one that comprizes the angle of elevation, along with the horizontal distance from the axis of hearing. When the sonorous object stands directly in front of the hearer, the semicircle ACB may be supposed to represent the horizontal section of his head, passing through the places of the ears, E and F, and the axis of hearing EF; also let G be the place of the sounding body, which, according to the conditions of the case, lies in the plane ACB produced, and likewise in the right line GS, which bisects EF at right angles: seeing then EF is bisected by the perpendicular SG, the arch ECF is also bisected by the same in the point C. Draw LG, GK, to touch the circle in T and P, then will the arcs TEC, and CFP be equal. Now all the pulses which do not move in right lines, contained in the angles TGS and SGP fly off without touching the circle; consequently they add nothing to the sound impressed on the ears by the body placed at G, whether the places E and F be supposed to lie in the arcs TC and CP, or without them. But the same number of pulses equal in force will fall in a given time, and in similar directions, on the arcs TEC and CFP as well as on the ears situated at E and F; and it is equally manifeſt, that the same reasoning will apply to two equal and similar solids, constructed upon the equal and similar planes, ECS and FCS. Now sound, though it be formed in the ears, is very much increased by the vibrations excited in the contiguous parts of the head by the pulses which fall upon them, as I shall endeavour to prove hereafter: therefore, as often as the two portions of the head, which are separated by the vertical plane perpendicular to the axis of hearing, are equally agitated by the pulses of the same sound, the ears are also equally affected from the same cause; which never happens, as we learn from the testimony of the other senses, unless the sounding body be placed somewhere in the right line that bisects the axis of hearing at right angles. In this manner men are taught by experience to draw a general inference from a general observation; they therefore conclude a body to be situated directly before or behind their persons, as often as the sound of it strikes both their ears with equal forces. The preceding demonstration elucidates the case of direct hearing, as far as this can be done by the assistance of a diagram; but the perception which determines the place of a sounding body to be in front of the hearer or behind him, requires a separate investigation; and I shall endeavour in the next place to ascertain the cause of it, by the following observations.

The head is a sensitive solid, and it perceives the impulses made on it by sounds much more exquisitely than men generally imagine. This sensibility is strongest in the auditory passages, and next to them in the parts immediately adjacent to the ears; nevertheless it diffuses itself more or less perfectly over the face, forehead, and temples, as well as all the external teguments of the skull. The sensation in question being of but little use independent of its connection with hearing, we for the most part mistake its true situation, and refer it to the organs of this sense, unless some circumstance, resembling the succeeding experiment, should happen to discover the nature of it to us. If any one will take the pains to close the orifices of his ears with wet paper, and will hold two slender rods of wood to his forehead or to one of his temples, taking care to keep the ends which are in contract with the skin separated by a small interval: and let another person at the same time touch the opposite ends of the rods with two watches, one of which does not move: the beats of the active watch will immediately pass along the stick, and make a sensible impression on the spot where its other extremity rests; which proves, that the bones of the head do not simply conduct sounds to the auditory nerves, but that the external teguments of this member also assist in discovering the directions of sounds by their sensibility. The same apparatus may be used to shew, that all parts of the head are not equally alive to the impulses of sounds; for a stick, which is of a proper length to impress the beats of a watch very faintly on the ear and parts adjacent, will prove too long to produce the same effect on the forehead, which is nevertheless much more exquisite in its feelings than the back part of the head.

The phænomena of oblique hearing remain to be explained; which case occurs as often as the sounding body is situated in the horizontal plane, but not in the right line that bisects the axis of hearing at right angles. Let M be the place of the sounding body, and draw MO to the centre of the circle; also let OC bisect the arc ECF, and take OG in it equal to OM: also draw WM, MR, PG, GI tangents to the circle. Now suppose a sound equal to that at M, to proceed from G, then the latter would have the same effect on the arc TCP that the former has on WDR, because the arcs are manifestly equal, and alike situated relative to the points M and G. But the sound proceeding from G is a case of direct hearing, consequently the ears placed at E and F receive equal impressions from it, which is not the case with the pulses that flow from M.

For though the forces imparted to the two arcs, TCP, WDR are equal, they do not fall equally on the circle in respect to the points E and F, which represent the ears; the sound therefore coming from M strikes these organs with unequal forces, as may be easily inferred from the figure.

What I have just now demonstrated by help of a semicircle may be said with equal justice of any solid, such as the human head, that can be divided vertically into two equal and similar portions by the plane, to which the axis of hearing is perpendicular. This assertion is too evident to need a laboured demonstration. It was observed in a former part of this paper, that if a person stop one of his ears, and hold a watch near it in a close room, he will hear the sound of it in the direction of the open ear by reflection, which circumstance may be sufficient to establish the truth of the preceding theory: but, in order to diversify the proof, the following experiment was tried. I took a wooden fork made of a stick of ash, which was split more than two-thirds of its length for the purpose; the points of the two branches turned inwards, and were placed at a proper distance to receive the head between them; a watch was then suspended upon the haft, or undivided part of the fork, after which the ends of the branches were brought into contact with two plugs of wet paper that closed my ears; in consequence of which I made the following conclusive remarks. When both points pressed with equal forces on the plugs of paper, I judged the watch to be directly before me from its beating: but as often as the pressure of one of the points was augmented, the sound seemed to quit its station in front of me, and to incline towards that side where the greater force was applied: lastly, I discovered, by using only one branch of the fork, that an increase of the pressure increased the effect of the same sound. The two cases of horizontal hearing being by this time pretty fully considered, it is necessary to change the subject, that the method whereby we judge of *elevation* by the ear may be examined in its turn.

For this purpose, suppose a right line to join the sonorous object, and the centre of the axis of hearing; also conceive a plane to pass through the same centre, to which the same right line is perpendicular. Then it is evident that all the pulses, which are impressed by the sonorous object on the head, must fall on that part of it which lies between the plane and the place of the sound; consequently that portion of the head is the seat of the sensation excited by the sonorous object; because the head is a sensitive solid, and capable of topical irritation arising from the impulses of sounds. Seeing now we are convinced, by a delicate sort of touch, what part of the head is affected by the strokes of the sonorous object, and are at the same time well acquainted with the form of this member of the human body, we judge accurately what is the situation of the excited portion of it, relative to the centre of the axis of hearing: in consequence of this judgment, we also determine the position of the plane last mentioned, which forms the base of the portion under consideration, by dividing it from the rest of the head.

But the right line which joins the middle point of the axis of hearing and the sonorous body, points out the direction of the sound, which line is perpendicular to the given plane in a given point; for which reason it is given in position: or, in other words, the direction of the sound in respect of fixed points in the hearer's head is determined: and this is done with a degree of accuracy, which I believe very few people are aware of. In order to satisfy my own curiosity on the subject, I tried an experiment, with a view to discover what is the least sensible variation of a given sound from the perpendicular to the axis of hearing, supposing the deviation to be measured by an arc of the horizon; and I found the angle comprehended by this perpendicular, and the sound's path, did not exceed eight degrees, The experiment was tried in an open plain, to prevent the intrusion of reflected sounds as much as possible. The instrument made use of was that which has been already described, as consisting of a bolt driven by a spring against a metal button; and the distance from my person to this sounding body was kept equal to forty feet, in the different parts of the experiment. I also discovered, by the assistance of the same apparatus, that the least sensible angle of elevation above the horizontal plane, was not more than ten degrees at the last mentioned distance; the sounding body being placed in the vertical plane, to which the axis of hearing is perpendicular. But the foregoing observations are not to be adopted and used as fixed rules, for much depends on the comparative sensibility of the auditory organ in different persons; and an alteration in the force of a sound will, without doubt, make a considerable change in the result of the experiment on elevation: because I have observed, that the feeble sound of a distant watch, though sufficient to shew whether it came from my right or left hand, was too weak to point out its situation, relative to the horizontal plane passing through my head. This imperfection in the sense of hearing, if it may be called one, is in all probability owing to the want of sensibility in the upper part of the head and lower part of the ſace, which defect renders them incapable of perceiving the impulses of ſeeble sounds.

The faculty of hearing which I have been investigating, betrays men under certain circumstances into errors, that appear the more surprizing, because the judgment relies on the admonition of the ears with the greatest confidence. The theory of these deceptions will therefore form a proper supplement to this essay. Mention has been already made of the sudden change that takes place in the sensible direction of a sound, as soon as the direct communication with the sounding body happens to be broken by the intervention of a lofty obstacle, provided the sound in question be loud enough to produce an echo from another quarter. Any person who has had occasion to walk along a valley obstructed with buildings, at the time that a peal of bells was ringing in it, will assent to the truth of the circumstance here alluded to. For the sound of the bells instead of arriving constantly, at the ears of a person so situated, in its true direction, is ſrequently reflected in a short time from two or three different places. These deceptions are in many cases so much diversified by the successive interpositions of fresh objects, that the steeple appears, in the hearer's judgment, to perform the part of an expert *ventriloquist* on a theatre, the extent of which is adapted to its own powers, and not to those of the human voice.

The phænomenon has often attracted my attention; and the similarity of effect which connects it with ventriloquism, convinces me every time I hear it, that what we know to be the cause in one instance is also the cause in the other: I mean that the echo reaches the ear, while the original sound is intercepted by *accident* in the case of the bells, but by *art* in the case of the ventriloquist. In order that the cause which gives rise to the amusing tricks of this uncommon talent may be pointed out with the greater clearness, it will be proper to describe certain circumstances that take place in the act of speaking, because the skill of the ventriloquist seems to consist in a peculiar management of them. Articulation is the art of modifying the sound of the larynx, by the assistance of the cavity of the mouth, the tongue, teeth, and lips. The different vibrations, which are excited by the joint operation of the several organs in action, pass along the bones and cartilages, from the parts in motion to the external teguments of the head, face, neck, and chest; from which, a succession of similar vibrations is imparted to the contiguous air, thereby converting the superior moiety of the speaker's body into an extensive seat of sound, contrary to general opinion, which supposes the passage of the voice to be confined to the opening of the lips: There are but few persons, I imagine, who have not some time or other witnessed an incident, which shews the vulgar notion to be erroneous in this particular. For if a man standing in a close apartment should happen to apply his face to a loop-hole, or narrow window, in order to speak to some person in the open air, a by-stander in the room with him will hear his voice, not indeed in its natural tone, but as if it were smothered by being forced to issue from a hollow case; but the circumstance of his words being heard distinctly, by one who cannot receive them from his mouth, proves the vibrations requisite for their production to be conveyed through the solid parts of the speaker's body, agreeably to the preceding assertion. The reason why we generally conclude the voice to be confined to the opening of the mouth, appears to be this. Those pulses which escape from the aperture are the strongest, they therefore surpass the weaker vibrations of the contiguous parts; for when a number of sounds moving in different directions strikes the ear at the same instant, the hearer does not notice their several places, but refers all of them to the quarter in which the most powerful is perceived. For instance, when a man stands at a sufficient distance from an extensive obstacle, his words are answered by an echo; but let him make a loud uninterrupted noise, neither he nor any body near him hears two voices whilst his continues, but as soon as the noise ceases the echo is perceived. This does not happen because the one begins the moment the other ends; but the reflected sound being the weaker of the two, it is smothered by that which precedes it.

We have seen in what manner secondary or reflected sounds are smothered by their principals; but though the places of such sounds are not recognized by the ear, their effects do not die away unnoticed: for the reverberated pulses mingle with those which come immediately from the sounding body, and thereby alter the sensation, which, without their interference, would be less compounded. This is the reason why the same musical instrument has one tone in a close chamber, where its notes undergo a multiplicity of reverberations, and another in the open air, where the reflections are few in comparison.

But it is time to apply the preceding facts to the subject in hand; and it will be proper to begin with a familiar example. When an orator addresses an audience in a lofty and spacious room, his voice is reflected from every point of the apartment, of which all present are made sensible by the confused noise that fills up every pause in his discourse; nevertheless every one knows the true place of the speaker, because his voice is the prevailing sound at the time. But were it possible to prevent his words from reaching any one of the audience directly, what then would follow? Undoubtedly a complete case of ventriloquism would be the consequence, and the person so circumstanced would transport the orator, in his own mind, to the place of the principal echo, which would perform the part of the prevailing sound at the instant. This he would be obliged to do, because the human judgment is bound, by the dictates of experience, to regard the person as inseparable from the voice; and the deception in question would be unavoidable, being produced by the same concurrence of causes which makes a peal of bells, situated in a valley, seem to change place in the opinion of a traveller. It is the business of a ventriloquist to amuse his admirers with tricks resembling the foregoing delusion; and it will be readily granted, that he has a subtle sense, highly corrected by experience, to manage, on which account the judgment must be cheated as well as the ear. This can only be accomplished by making the pulses, constituting his words, strike the heads of his hearers, not in the right lines that join their persons and his. He must therefore know how to disguise the true direction of his voice, because the artifice will give him an opportunity to substitute almost any echo he chuses in the place of it. But the superior part of the human body has been already proved to form an extensive seat of sound, from every point of which the two pulses are repelled, as if they diverged from a common centre. This is the reason why people, who speak in the usual way, cannot conceal the direction of their voices, which in reality fly off towards all points at the same instant. The ventriloquist therefore, by some means or other, acquires the difficult habit of contracting the field of sound within the compass of his lips, which enables him to confine the real path of his voice to narrow limits. For he, who is master of the art, has nothing to do but to place his mouth obliquely to the company; and to dart his words, if I may use the expression, against an opposing object, whence they will be reflected immediately, so as to strike the ears of the audience from an unexpected quarter, in consequence of which the reflector will appear to be the speaker. Nature seems to fix no bounds to this kind of deceptions, only care must be taken not to let the path of the direct pulses pass too near the head of the person who is to be played upon; for, if a line joining the exhibitor's mouth and the reflecting body approach one of his ears too nearly, the divergency of the pulses will make him perceive the voice itself instead of the reverberated sound.

The only ventriloquist I ever attended, acted in strict conformity to the preceding theory of this curious paradox in the science of acoustics. His audience was arranged in two opposite lines, corresponding to the two sides of a long narrow room. The benches on which they were seated reached from one end of the place to the middle of it, the other part remaining unoccupied. The feats exhibited by him were the three following. *First:* he made his voice come from behind his audience, but *it never seemed to* proceed from any part of the wall, near the heads of the people present; on the contrary, it was always heard resembling the voice of a child, who seemed to be under the benches. He stood during the time of speaking in a stooping posture, having his mouth turned towards the place from which the sound issued; so that the line joining his lips and the reflecting object, did not approach the ears of the company. *Second:* advancing into the vacant part of the room, and turning his back to the audience, he made a variety of noises, that seemed to proceed from an open cupboard which stood directly before him, at the distance of two or three yards. *Third:* he placed an inverted glass cup on the hands of his hearers, and then imitated the cries of a child confined in it. His method of doing it was this; the upper part of the hearer's arm laid close along his side; then the part below the elbow was kept in an horizontal position with the hand turned downwards, which was done by the operator himself. After taking these preparatory steps, the man bent his body forwards in a situation which presented the profile of his face nearly to the front of his hearer, whilst his mouth pointed to the cup; in which posture he copied the voice of a confined child so completely, that three positions of the glass were easily distinguished by as many different tones, viz. when he pressed the mouth of the cup close against the palm, when one edge of it was elevated, and when the vessel was held near the hand but did not touch it. The second and third instances of ventriloquism afford strong proofs, that this delusive talent is nothing more than the art of substituting an echo for the primary sound; for, besides the change perceivable in the direction of the voice, it was found to be blended with a variety of secondary sounds; such as we know by experience are produced as often as a noise of any kind issues from a cavity. I have already made some remarks on this species of knowledge; but it would be improper to dismiss the subject without noticing the accuracy, with which the ear recognizes the finer modifications of sounds, and their causes. I have frequently observed, that a certain waterfall makes a flatter and duller noise when the ground is covered with snow, than that which it affords at other seasons. The human voice also undergoes a similar change within doors, by striking a multiplicity of soft bodies, such as a number of piles of wool, or a crowded congregation in a church.

The method of preventing the vibration of the vocal organs from reaching the external teguments, is still wanting to complete this theory of ventriloquism; and I presume *it* can only be supplied by an adept in the art. I must therefore dismiss the subject unfinished, because I have no pretension to that character.

The THEORY *of* COMPOUND SOUNDS.

BY MR. JOHN GOUGH.

COMMUNICATED BY DR. HOLME.

Dr. Smith, author of the work on Harmonics, takes for granted in his theory of compound sounds, that the pulses which proceed at the same time from a number of sounding bodies, do not clash, or obstruct one another, in their passage through the air. According to this hypothesis, each set, of any number of cotemporary sets of pulses, strikes the ear without being confounded with the rest; in consequence of which, any number of sounds may be distinctly perceived at the same time. On this supposition, a compound sound is a sensation rendered variable by the irregular manner in which the pulses of the constituent sounds succeed to one another. For, if the intervals of time between two successive pulses of one of the constituent sounds be not equal to the same intervals belonging to the sound or sounds which accompany it, the secondary intervals, or small parts of time separating the pulses which fall in succession on the ear, will vary in magnitude; in the same manner that the distances between the figures upon the face of a barometer and its nonius vary, none on the slider coinciding with those on the fixed plate excepting the highest and lowest. I have chosen this familiar instrument to illustrate Dr. Smith's method of explaining the physical cause of compound sounds, because it affords a visible example of a cycle of pulses, according to his notion of the subject.

The sketch which I have exhibited of Dr. Smith's hypothesis *shews*, that he allowed that a number of *simple sounds* might exist in concert, and strike the ear in a distinct manner, without suffering any interruption in their motions from the interference of their pulses. But a late writer on sound rejects this axiom in Harmonics as a mathematical inconsistency; and substitutes the following theory of compound sounds in the room of it. If two musical strings, differing in their times of vibration, happen to vibrate in concert, they do not occasion two distinct sounds in the opinion of this gentleman, because the strings agitate the air in conjunction; consequently the pulses, which one of them would actually form in an undisturbed atmosphere, must unavoidably clash with those which the other string would produce in similar circumstances. Hence the waves of air belonging to both strings are interrupted in their natural progress, and are compelled by their mutual interference to coalesce, thereby producing a new succession of pulses, constituting a single sound in the place of two. This sound is of a peculiar kind; for, the pulses of which it consists, are separated by unequal intervals of time, and disposed in cycles.

The merit of the preceding theory, when compared with Dr. Smith's hypothesis, must be ascertained by contrasting it with a variety of facts, which are furnished by the phænomena of compound sounds, and make a part of every man's experience. For, if it be found upon examination to be repugnant to these facts, it will prove inconsistent with nature, and cannot fail of disappointing the inventor's expectations.

Were it possible for a number of sounds to coalesce, and form but one, the compound would acquire sensible properties peculiar to itself, and at the same time lose the distinguishing characters of its elements, some of which are incompatible with the qualities of an individual. On this supposition, the presence of the constituent sounds could not be detected by the ear in this newly created being: on the contrary, an experimental process would be required to analyse every compound sound the first time it attracted a man's attention, for the same reason that a chemist finds it necessary to analyse a substance with which he is unacquainted. The abstract term *coalescence* is used, in a physical sense, to signify any intimate union of bodies or the powers of bodies; and the introduction of the term into language proves the existence of the principle in nature, or more properly in the human mind. For, when a number of agents act in conjunction upon one of the senses, we have two ways of conceiving their mode of operation. If the sensible effects of each agent be distinctly perceived, we attribute a separate action to every member of the assemblage, and call the aggregate a mixture: this is the conclusion of a person who tastes an infusion of pepper in vinegar. On the other hand, when we know that certain agents are present without being able to recognize their distinguishing powers, in the room of which we find qualities of a different description, we pronounce the aggregate to be in a state of coalescence. This is the situation of the chemist, who tastes common salt, but cannot perceive the presence of soda and the muriatic acid. It is my business then to prove compound sounds to be *mixtures*, not aggregates by coalescence. This I shall endeavour to do, by shewing that they have properties which belong not to individuals, such as a number of tones, a variety of directions, and several sets of pulses.

First, the tones of a flute and violin are as distinct to sense as any two things can be when they are sounded separately; and I appeal to common experience to determine, if they are not equally distinct when heard in concert. Taking it for granted that the answer will be in the affirmative, I pronounce the aggregate to be a mixture of sounds in one case. Secondly, if a violin sound in front of the hearer, and a flute be heard at the same time in an oblique situation, the person thus circumstanced is able to determine the relative positions of the two instruments, which shews the aggregate to have two cotemporary directions. It is therefore a mixture of sounds, not a single sound. Thirdly, I have found by making the experiment, that any number of musical strings may be made to vibrate by a compound sound acting upon them, provided this compound be occasioned by an equal number of strings with the former, having one in the latter set in unison with each one in the preceding set; This is an experimental proof that there are as many sets of pulses in an aggregate of sounds as that aggregate contains elements, because no string whatever is in unison with a concord or discord. Lastly, if it were possible for sounds to coalesce, men would never hear any thing more than one noise at one time: The general hum would have varied perpetually from the extinction of existing sounds, and the intrusion of fresh ones; but the human mind would have had no conception of two cotemporary sounds; because the ear being in that case incapable of conveying the complex sensation, the idea of such an existence would have transgressed the sphere of human knowledge. The preceding arguments are drawn, for the most part, from common experience; and they shew, that the free passage of cotemporary sounds through the air may be safely admitted as an axiom in harmonies. I shall therefore proceed to prove the same *proposition to be consistent* with the doctrine of forces.

The propagation of sound through the atmosphere, and the nature of aëreal pulses are commonly explained in elementary books of natural philosophy; I shall, for this reason, enumerate only a few particulars, the recollection of which will be found useful.

Proposition I. Two contiguous particles of air which are agitated by a vibrating body, either directly or by the intervention of an elastic medium, receive two motions from each impulse; *first*, an absolute motion carries them to a greater distance from the sounding body, and afterwards brings them towards it again, both the progress and regress being performed in the time of a single vibration: *second*, a relative motion resulting from the former, compels the two particles to approach and recede alternately, which double motion is also accomplished in the time of a single vibration.

PROPOSITION II. Both the absolute and relative motions are greatest amongst those particles which are nearest the sounding body, and they diminish as the distance from that body increases; but, in all cases, the change of place is too small to be perceived by the ear, on which account every particle preserves a fixed position in respect of this organ and its connections.

For each corpuscle is confined within the circumference of a physical right line, the diameter of which is determined by its own absolute motion.

PROPOSITION III. If two sounding bodies, affording different notes, act in conjunction upon the same particle, through the media of two right lines of similar particles connecting them with it, this particle will be urged at the same instant in the direction of these lines, by two forces having an inconstant ratio.

For, let the particle A be urged, by the acuter sound, in the line SA, and by the graver, in the the line TA; (*vide* fig. 2, plate 9, page 636.); then the contiguous particle V, placed in SA, will approach to, and recede from A more frequently than W, similarly placed in TA, by Prop. I.: consequently the force of V upon A will vary in a quicker manner than the force of W upon A; but this variation of ratio is limited in time; because it evidently begins and ends with the cycle of the vibrations of the sounding bodies.

Proposition IV. The coalescence of two sounds is impossible on mechanical principles.

For, suppose the thing possible; then the coalescence of two sounds requires, that a particle of air should possess a motion, compounded of the motions which the two sounding bodies would impart to it separately; and that this compound motion should act in a given right line, for an assignable part of time, otherwise it could not excite a similar motion in the elastic particles occupying that given right line. Let A be such a particle, and let the construction used in the last proposition, be retained; consequently (Principia, Prop. 23, Lib. 2.) VA and AW are in the ratio of the forces that act at any moment in the right lines TA and SA. Make AK as AW, and draw KL parallel to AW, and make it as AV; also join AL; then will the particle A be urged in the direction LA at that instant. But the ratio of AK to KL varies perpetually, by Prop. III.; therefore the species of the triangle AKL is equally inconstant; consequently the compound force does not act in a given direction for an assignable part of time. Now the production and propagation of motion in a given right line requires force to be combined with time, which combination is wanting in the present instance; wherefore the coalescence of sounds is impossible.

Proposition V. It may be demonstrated from mechanical principles, that a number of distinct cotemporary sounds cannot do otherwise than produce distinct sensations.

In order to make the necessary diagram as simple as possible, let the directions of two cotemporary sets of pulses be represented by the right lines SM and TN, lying in the same horizontal plane, and intersecting in the point A; also, let BCD be the horizontal section of the hearer's head, made by the same plane; and suppose the centre of the axis of hearing to be at O; draw OM, ON perpendicular to SM, TN. Now I have shewn in the preceding paper, that if a set of pulses move in either of the right lines SM, TN, it will excite a sensation in that part of the head which is cut off by a vertical plane, passing through one of the perpendiculars OM, ON. It also appears from the last proposition, that the impulses of the vibrating bodies, acting in the lines SA, TA, do not compel the particle A to move in any given intermediate direction, as LA. But, according to the second proposition, the position of the particle A, is fixed in respect of the planes MO, NO; that is, though the corpuscle actually changes place, in respect of the geometrical point A, it is always found in the intersection of the physical right lines SM, TN. Now the two vibrating bodies continue to act in the directions of these right lines, consequently the particle A is constantly urged in these lines by two forces, which, though variable in magnitude, are combined with time; which circumstance enables the corpuscle to transmit the impulses of one body to M, and those of the other to N. What has been demonstrated of the particle A, may be affirmed of any other particle, which is the intersection of two right lines parallel to SM, TN; in other words, it may be affirmed of two sets of pulses; and the same demonstration may be extended to three sets, &c.

Corollary 1. The substance of this and the preceding proposition will apply to all elastic mediums; hence it happens, that a plate of glass &c. in a state of vibration, will conduct a foreign sound, whilst it produces one of its own; for the same reason, if light be considered as a vibrating medium, one particle of the luminous fluid may assist in transmitting two sensations.

Corollary 2. When the inclination of the planes MO, NO, is less than a given angle, the ear cannot distinguish the relative positions of the sounding bodies; it therefore refers them to the same place.

The first time I perused Dr. Smith's Harmonics, Dr. Young's objection occurred to me; but the preceding train of arguments removed the scruple, without discovering the author's reasons for treating this article of his work with so much brevity. Perhaps the demonstration, which cost me an effort of study, was an intuitive conclusion in his comprehensive mind. As soon as the proposition was established, I assented to his definition of an interval of sound, allowing it to be a quantity of a certain kind, terminated by a graver and an acuter sound. The demonstration of Prop. V. convinced me, that intervals of this sort may be subdivided by the interposition of one or more intermediate sounds, which concession formed the basis of my analysis of the human voice.* Speculative men may differ in opinion about the origin of the small intervals which form the tones of various voices; but they must exist, whether we ascribe them to an undulating motion like that of a stretched cord, or to the cotemporary vibrations of a system of elastic bodies. It does not appear, that Dr. Young was acquainted with my paper at the time he composed his own; but he found it necessary to allow the tone of the larynx to receive various modifications from the vibrations of the adjacent

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* Page 58.

parts. His theory therefore differs from mine in this particular only: he pronounces the voice to be a compound by coalescence; I deny the possibility of such a compound, and call it a mixture of imperfect unisons. This mixture appears to be a single sound, because it has but one direction; for the proximity of the various parts contributing to the formation of it, disqualifies the ear, so that it cannot perceive their relative positions, and compels it to refer them all to one place, *by Corollary 2 to proposition* V.

A certain class of sounds, which, for the sake of brevity, were not noticed in my paper on the voice, deserve a place in the present communication. If a finely-toothed file pass slowly over a smooth elastic substance, such as a piece of horn, it makes a grating noise; but if the velocity of the instrument be sufficiently increased, a continued sound is produced, which becomes more or less acute, by giving a quicker or slower motion to the file. The grating noise is occasioned by a succession of short interrupted sounds, resulting from the united vibrations of the file and the body it scratches; but the quick succession of these sounds, caused by an increase of velocity, gives rise to a secondary sound resembling the harmonical notes, being produced by a like cause. Now this sound becomes a primary object with the ear, in all probability because the pitch of it may be varied; for the first sounds proceeding from the action of the file, evidently supply nothing but the tone. Many instances of the kind occur in art and nature: the notes of all reed-instruments are of this description, and the voice must be referred to the same class, because the larynx resembles a reed-instrument in structure.

METEOROLOGICAL OBSERVATIONS.

BY JOHN DALTON.

Observations on the Barometer, made at Manchester, for 1801.

	Mean.	Highest.	Lowest.
Jan.	29.59	30.05	28.98
Feb.	29.56	30.02	28.87
Mar.	29.61	30.20	28.68
April	29.86	30.20	29.10
May	29.65	30.00	29.20
June	29.88	30.11	29.53
July	29.65	30.10	29.13
Aug.	29.88	30.15	29.42
Sept.	29.73	30.12	29.11
Oct.	29.62	30.15	28.73
Nov.	29.55	30.07	28.68
Dec.	29.29	39.00	28.51
	29.66	Mean for the year.	

The observations were made three times a day.

It is now well known that the fluctuations of the barometer are not local, but extend over a considerable portion of the earth: the extremes, whether high or low, usually take place on the same day in Great Britain, France, Germany, and Russia, and seldom differ more than one day. If a number of barometers were stationed at equal distances over the surface of the globe, and cotemporary observations made on them for a year or more, we should then probably be in possession of facts from which a rational theory of the variation of the barometer might be derived. Observations made in different parts of the same province or country seem not now of much importance in this respect.

Observations on the Thermometer, made at Manchester, for 1801.

	Mean.	Highest.	Lowest.
Jan.	39°·3	52°	23°
Feb.	39·4	52	28
Mar.	42	57	27
April	46.5	68	28
May	51·9	67	38
June	56.3	73	40
July	58*	—	—
Aug.	62.1	80	53
Sept.	56	67	47
Oct.	49·2	60	35
Nov.	39·6	54	26
Dec.	34·6	45	20
Mean	48		

The observations were made three times a day; namely about 8 A. M. and 1 and 11 P. M. The mean obtained from them is probably below the true mean temperature.

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* The observations in July were interrupted.

The mean of the morning observations through the year was 46°.5; at 1 P. M. 52°.5; at 11 P. M. 45°: the mean taken upon the 1 and 11 o'clock observations would be nearly 50°, which agrees with the temperature of most springs in this place.

Account of Rain at Manchester.

	1794, Inch.	1795, Inch.	1796, Inch.	1797, Inch.	1798, Inch.	1799, Inch.	1800, Inch.	1801, Inch.	Mean, Inch.
Jan.	1. 09	0. 95	3. 10	1. 58	2. 70	1. 76	2. 86	2. 42	2. 06
Feb.	3. 77	2. 46	2. 29	1. 21	1. 91	4. 16	0. 44	1. 85	2. 26
Mar.	2. 03	2. 02	0. 58	0. 94	1. 18	2. 16	2. 37	2. 89	1. 77
April	3. 91	3. 23	1. 60	2. 18	1. 37	2. 19	4. 10	0. 79	2. 42
May	2. 27	1. 01	5. 09	5. 96	1. 48	2. 13	2. 85	2. 60	2. 92
June	1. 25	2. 06	2. 35	4. 26	0. 85	2. 05	0. 71	0. 53	1. 76
July	3. 40	3. 01	5. 19	2. 52	4. 75	4. 08	0. 29	4. 85	3. 51
Aug.	4. 38	4. 28	1. 04	5. 49	4. 13	8. 74	1. 05	0. 73	3. 73
Sept.	4. 66	0. 46	2. 63	3. 86	3. 35	5. 35	6. 55	6. 41	4. 16
Oct.	5. 10	5. 49	2. 87	2. 31	3. 51	3. 77	4. 37	4. 27	3. 96
Nov.	3. 45	4. 16	2. 13	2. 99	3. 68	1. 90	3. 70	3. 15	3. 14
Dec.	2. 11	3. 61	1. 76	5. 50	2. 35	0. 35	3. 05	4. 56	2. 91
	37. 42	32. 74	30. 63	38. 80	31. 26	38. 64	32. 34	35. 05	34. 60

Hence the annual quantity of rain on an average of 8 years, is 34. 60 inches.

Mr. George Walker of Salford, makes the average rain for the same time 38. 5 inches. He seems to think his measure has somewhat over-rated the quantity; and it is not improbable mine may be too little: add to this, the gages are a mile and a half distant, and mine on more elevated ground.

There were 160 days in the year at an average, on which rain more or less fell.

I kept a rain-gage on the top of St. John's steeple, Manchester, from midsummer 1797 to the end of 1798; and another on the ground in the vicinity, about 50 yards perpendicularly below. In summer the ratio of the rain below to that above was 3 : 2 nearly; in winter it was 2 : 1 nearly.

Rain at Kendal, 1793—52. 74; 1794—69. 04; 1795—56. 25; 1796—45. 73; 1797—56. 83; 1798—54. 63; 1799—56. 93; 1800—48. 20; 1801—50. 612; uniting these observations with those for the five preceding years (see my Meteor. Essays) we obtain 58.1 inches the mean annual rain at Kendal for 14 years.

Observations on the Winds for 1801.

North, 44: North East, 277: East, 11: South East, 19: South, 22: South West, 412: West, 153: North West, 12. Total 900.

The South West and the North East winds, as usual, have been the most prevalent; they are in fact winds that properly belong to the northern temperate zone, arising from the two general currents of air tending from and towards the equator. (See my Meteorological Essays, page 91).

Account of the Evaporation from Water, in a Cylindrical Vessel of 10 *inches diameter, kept nearly full, at Manchester.*

	1799. Inch.	1800. Inch.	1801. Inch.	Mean Evap. Inch.
Jan.	———	———	———	1. 5 *
Feb.	———	———	———	2. 0 *
Mar.	1. 082	3. 700	———	3. 5 *
April	5. 398	4. 760	———	4. 5 *
May	5. 050	5. 228	4. 600	4. 959
June	7. 702	5. 207	6. 551	6. 487
July	5. 157	5. 679	6. 048	5. 628
Aug.	6. 000	6. 376	5. 798	6. 058
Sept.	4. 340	3. 986	3. 368	3. 898
Oct.	3. 337	1. 998	1. 718	2. 351
Nov.	2. 428	1. 600	2. 098	2. 042
Dec.	1. 384	———	———	1. 5 *
				44. 4 Ann. Ev.

N. B. Those means marked * are conjectural; the frost in the winter season usually damaged the gage.

The mean annual evaporation from soil covered with grass, or from green ground, exclusive of dew, was at an average for the three years above, 23½ inches. (See page 361 for the three years preceding.)

Observations relating to Hygrometry.

One important attainment in meteorology is the knowledge of the quantity of aqueous vapour existing in the atmosphere at any time. From this, and the observed temperature of the air, we can easily ascertain its disposition for the evaporation or precipitation of water; or in other words for *fair* or *rainy* weather. Various instruments have been used to determine the quantity of vapour in the air. The hygrometer of De Saussure seems to gain the most credit; but I have in a former essay assigned a reason for disallowing it. It is acknowledged that an hygrometer ought to indicate that degree of cold which is necessary to make the air begin to part with vapour, or to form dew upon the surfaces of bodies; now as this is at all times capable of being effected by a simple experiment, (see page 581) it almost totally supersedes the necessity of a dubious, very delicate, and consequently easily injured instrument.

In my journal for a year and a half past, I have had a column entitled, *vapour point*, in lieu of the hygrometer column. By vapour point I mean that degree of the thermometer at which dew begins to be formed at the time. The higher this point is, the greater is the quantity and force of vapour in the atmosphere, as is shewn by the table at page 559; and the lower it is with respect to the actual temperature of the atmosphere, the greater is the force of evaporation.

1800. *July.* Mean vapour point for 21 days =53°; highest 62°; lowest 40°.

Aug. Mean for 11 days =56°; but too high for the monthly mean: highest 60°.

Sept. Vapour point above 50° for 6 days; highest 60°.

Octo. Vapour point mostly below 52°; highest 59°.

1801. *May.* Vapour point above 50° for 4 days; highest 55°.

June. Mean for 10 days, 49½°; highest, 57½°; lowest, 30°*.

July. Mean for 8 days, 53°; highest, 56°.

Aug. Mean for 22 days, 54½°; highest, 61°.

Sept. Mean for 14 days, 54°; highest, 60°.

Octo. Vapour point for 5 days, above 50°; highest, 57°.

Nov. Highest, 54°; lowest, 22°.

Dec. Highest, 44°; lowest, 18°.

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* On the 13th. great damage done to potatoes, &c. by the cold which accompanied this remarkably low state of vapour for the season. It was 46° on the 12th. and 40° on the 14th.

The mean monthly vapour point may also be inferred from the monthly evaporation and mean monthly temperature, according to the theory in the Essay, page 574.—Let August be taken for an instance: the evaporation was 5,798 inches= 1312 grains from my small gage, which is nearly 6 inches in diameter: but 1312 ÷ 1440 (= min. in 24 hours) = .91 gr. per minute, the mean rate of evaporation. The mean temperature (taken from the mean noon and evening observations) was 63°, with which entering table at page 587, we obtain 3. 63 grains, the evaporating force at that temperature (taken from the third column, because the evaporating gage is in a very open situation). Subtracting .91, there remains 2. 72 grains, corresponding in the said table to 54°. 3, the mean vapour point. The mean derived from actual observation is stated above to be 54½°.

AURORÆ BOREALES:

Observed since 1793*.

1794. January, 7 and 22. March, 8 and 29. December, 8 and 19.—Total, 6.
1795. September, 8 and 14.—Total, 2.
1796. None observed.

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* For those observed by me preceding that period, see my Meteorol. Obs. page 54.

1797. January, 22. Feb. 1, 18, 27 and 28. March 2 and 10. April, 24. Nov. 18, 21, 22 and 23. Dec. 20.—Total, 13.

1798. None observed.

1799. September, 3. Octo. 25.—Total, 2.

1800. March, 18. Nov. 2 and 7. Dec. 10.—Total, 4.

1801. January, 4 and 25. Feb. 22. Aug. 18. Octo. 6.—Total, 5.

In all, 32.

The *Auroræ* have been much less frequent in the above period than for the same number of years before.

I observed 53 of them in 1788.

All the phenomena corroborated the notion maintained in my Essays abovementioned; namely, that the luminous beams of the *Auroræ* are cylindrical, magnetic, parallel to each other and to the dipping needle. The centre of each *Aurora* uniformly appeared to be in the magnetic north.

APPENDIX

I.

Explanation of a Roman Inscription by Mr. THOMAS BARRITT: *with a Note on the same subject by* Dr. HOLME.

The stone (see plate VII.) found in the year 1795 in the Castle-field Manchester, (the Mancunium of the Romans) from what appears of the inscription, seems to have been a votive one, dedicated to Jupiter, by the first Frisian Cohort stationed there, in the 24th year after their arrival.

Camden mentions two inscriptions at Manchester, found in the old Mancunium; one he says he saw himself, and another was copied for him by the famous mathematician, Dr. Dee, warden of Manchester College. They were placed there in memory of two Centurions, who in their turn had commanded the Frisians, under the Roman government, for 23 years.

The Frisian Cohort at Mancunium is supposed to have been part of, and to have belonged to the sixth Roman legion which was stationed at York, and stiled Victrix; but it may admit of a doubt whether this cohort did not belong to the 20th legion, stationed at Chester, and likewise stiled Victrix.

In Archæologia, vol. 3, page 236, the late learned and Rev. John Watson, rector of Stockport, exhibited a drawing and description of a stone similar in size, shape, and grit to this of Mr. White's; it was discovered at Melandra Castle, a little way from Mottram Longendale; the ornaments and two first lines correspond very much with Mr. White's; but upon viewing the stone I found the two last lines very imperfect: yet it must be confessed the superior judgment of Mr. Watson was alone of sufficient authority to establish the interpretation. He read it

Cohortis primæ Frisianorum Centurio Valerius Vitalis.

This Valerius perhaps might not only belong to the first Frisian Cohort, but be the first centurion, or commander of an hundred men, placed there at the erection of the castle, and the stone fixed up in memory thereof at his own request.

It appears likely the stone was fixed over the centre of the arched gateway of the castle, it being found in the ground where the principal entrance into the fortress was situated; and Mr. Watson's stone was discovered in the gateway at Melandra Castle. This at Manchester was probably thus placed when the gate was erected, or at least when it might undergo a repair in the time of Trajan or Hadrian, coins of both emperors being found at the place where the gate stood.

This castle was in a ruinous state about the year 900: history says that Edward the elder, king of the West Saxons and afterwards of the Mercians, sent an army of the latter into Northumberland, which then had a king of its own, to repair the castle at Manchester, and put a garrison into it, as a defence against the Danes, who were ravaging the kingdom with fire and sword.

Note by DR. HOLME.

The following is, I apprehend, a more correct transcript of the inscription, than that exhibited in plate VII. page 534. The characters I have ventured to supply in italics, are obscure in the original. In the engraved copy an O is substituted for the Q in the fourth line; and there is a member redundant in the complication that follows it.

CHOR. I.
FRISIAVO
N. Q. VI. MVNI.
M. P. XXIIII.

Probably:

Cohortis primæ Frisiavonum quæ viam munivit millium passuum viginti quatuor: which may refer to the construction of the military road between MANCUNIUM and CONDATE; as the distance between these stations, fixed by Richard of Cirencester in his tenth *Iter* at twenty-three miles, measures, according to Mr. Whitaker, twenty-two English, which are nearly equivalent to twenty-three Roman miles and three quarters.*

The relic before us is of importance, as it enables us to restore the proper appellation of the cohort that garrisoned Mancunium: concerning which antiquarians have been misled by an ambiguous contraction in the inscription at Melandra Castle, and probably in that transcribed for Camden by Dr. Dee. It is farther valuable, as it may serve to vindicate the authority of Pliny and the purity of his text, in regard to a subject on which they have been questioned, in a work of great erudition published by an eminent scholar of the seventeenth century[*].

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* Hist. of Manchester, I. 102.

The Frisiabones, or adopting the reading of Harduin's MSS. Frisiavones, are twice mentioned by the elder Pliny: first as inhabitants of an island situated at the mouth of the Rhine, between the Maese and the Zuyder Zee; and secondly as a nation of Belgic Gaul[†]. The former are supposed by Harduin to have been a body of emigrants from the latter. The name is likewise preserved in an inscription found at Rome, of which I insert a copy from Gruter[§].

D. M.
T. FL. VERINO.
NAT. FRISÆVONE.
VIX. AN. XX. M. VII.
T. FL. VICTOR.
EQ. SING. AVG. FRATRI.
DVLCISSIMO.
F. C.

Whether the Mancunian cohort was the same with the *Cohors I. Frixagorum* of the *Notitia*, stationed in the decline of the empire at *Vindobala*, is a question that must be decided by future discoveries; as no inscriptions occur at Rutchester, which is supposed by Mr. Horsley to coincide with that station.

E. H.

February 24, 1802.

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[*] *Vid.* Cluverii German. Antiq. 561.

[†] Hist. Nat. Lib. IV. capp. 29. 31.

[§] Inscript. Antiq. DXXXII. 7.

APPENDIX.

II.

Note to MR. W. HENRY'S *Paper on Heat.*

The argument, at page 611, which is the basis of my objections to the commonly employed mode of ascertaining specific caloric, I fear is not so fully and clearly stated, as the abstruse nature of the subject requires.

Assuming two bodies, A and B, to be at the point of privation of temperature, or to possess no free caloric whatsoever, the quantity of combined caloric in each, according to Dr. Crawford's theory, is *directly* proportional to the quantities of heat, necessary to produce equal elevations of temperature in the two bodies. Thus, if to attain a given temperature, A require caloric as 20, and B only as 10, the combined caloric of A, before this addition, is inferred to have borne to that of B, the ratio of 2 to 1. But it might, with equal or perhaps greater probability, have been assumed, that the combined caloric of A and B is *inversely* proportional to the quantities of heat, required to produce a given temperature;—that A, for example, to attain a certain temperature, has absorbed more caloric than B, because in A less caloric existed, previously, in a state of chemical union.

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INDEX.

A

PAGE.

Air, on the velocity of, issuing out of vessels in various circumstances, 398—admitted into an exhausted receiver acquires an increase of 50° in temperature, 520.—condensation of it to a double density increases its temperature 50°, and liberating it again diminishes its temperature 50°, - - - - - - 525

Analysis, of two mineral springs near Warwick, - 174

Anderson, Dr. on an universal character, - 89

Antiques, account of some lately found in the river Ribble, - - - - - - 527

Atmosphere, supposed to be constituted of several elastic fluids, co-existing, but independent of each other, 545—Lavoisier's description of it too limited, 548—*aqueous*, force of it determinable from its condensation on a glass containing a cold liquid, - - 581

Auroræ boreales, observed at Manchester from 1793 to 1802, - - - - 673

B

Banks, Mr. on the velocity of air issuing out of vessels, &c. - - - - - - 398

Bardsley, Dr. on party-prejudice, - - 1

Barritt's, Mr. account of some antiques, - 527

PAGE.

Barometer, observations on it at Manchester, - 666

Beautiful, in the human form, essay on, 407—our idea of it derived from contemplating a medium form, 427

Bellows, an instrument to measure the force of their blast, - - - - - - - 404

Benefits, and duties of Literary and Philosophical Societies, essay on, - - - 70

Betula nigra, or *Birch*, black American, recommended for quick growth, &c. - - - 164

Bleaching, a new method of, - - 298

BROWN, Dr. on an universal written character, - 275

C

Caloric, materiality of it supported, 609, *et seq.*—seems subject to the laws of chemical affinity, - 615

Celts, account of some, lately found in the river Ribble, - - - - 527

Cold (absolute) or the point of total privation of heat, new method of inferring it, - - 601

COLLIER, Mr. on iron and steel, 109—on fermentation, and distillation of ardent spirit, - - 243

Colour, of a Negro, remarkable change in, - 314

Colours, instances of extraordinary vision of, - 28

Complexion, florid, appears to some people, blue, 41

Cotton-tree, (Persian) an account of, - - 214

D

DALTON, Mr. on extraordinary vision of colours, 28—on the quantity of rain, compared with evaporation, &c. 346—on the origin of springs, 367—on the power of fluids to conduct heat, 373—on the heat and cold produced by the mechanical condensation and rarefaction of air, 515—on the constitution of mixed gases, 538—on the force of steam in *vacuo*, 550—on

PAGE.

the force of steam in air, 571—on evaporation, 574—on the expansion of elastic fluids by heat, - - 595

Detonation of oxygenated muriate of potash and combustible substances, - - - 233

Dew, estimate of the annual quantity of, 350—on glass, &c. a criterion of the force and quantity of aqueous vapour in the atmosphere, - - - 582

Distillation of ardent spirit, on the, - - 243

E

Ether (sulphuric), vapour from it subject to the same law of force as that from water, 365—force of its vapour at 212° of temperature, 567—evaporation of it in the open air, - - - - - 590

Evaporation, mean monthly from water, at Liverpool, 359—mean monthly from green ground, at Manchester, 361—from water in every temperature is directly proportional to the force of steam of the same temperature, 580—table for determining it in atmospheric temperatures, 585—from spirits, ether, &c. follows the same law as from water, 590—from ice, also is subject to the same law, 593—from water, at Manchester, for 3 years, - - - - 670

Expansion of elastic fluids by heat, 595—is the same for all elastic fluids, contrary to the experiments of du Vernois, 600—amounts to 325 parts from 1000 from 55° to 212°, *ibid.*—increments of it *decrease* in ascending, compared with equal increments of mercury, 599

F

Fermentation, experiments and observations on, - 243

Fetus (acephalous), account of one, 498—another, 502

Forces (central), inverse method of, - - 402

G

PAGE.

Gases (mixed), on their constitution, 535—not resulting from chemical affinity, - - - 539

GISBORNE, Rev. Thomas, on the benefits and duties of literary and philosophical societies - - 70

GOUGH, Mr. on the variety of voices, 58—on the method of determining by the ear the direction of sonorous bodies, 622—on the theory of compound sounds, - - - - - 653

Grass, colour of, nearly the same as that of red sealing-wax, to some eyes, - - - 41

GUTHRIE, Dr. on the Persian cotton-tree, - 214

H

Heat, the power of fluids to conduct it independent of internal motion maintained, 373—materiality of it supported, - - - 609, *et seq.*

HENRY, Mr. on certain experiments supposed to disprove the materiality of heat, - - - 603

Hints, on the establishment of an universal written character, - - - - - 275

HOLME's, Dr. explanation of a Roman inscription, 677

HOYLE, Mr. on the oxygenated muriate of potash, 221

HULL, Dr. on the nervous systems of different animals, and original defects in them, - - 475

Huln Abbey, Northumberland, enquiry into the name of its founder, - - - - 46

Hygrometer of de Saussure, remarks on it, - 595

Hygrometry, observations relating to, - - 671

I

Ice, a bad conductor of heat, 388—is evaporable, like water, - - - - - 593

Iron and Steel, observations on, - - - 109

Inscription, (Roman) found near Manchester, explanation of, - - - - - 675

L

PAGE.

Lambe, Dr. on the analysis of two mineral waters, 174

Lavoisier's description of the atmosphere too limited, - - - - - 548

Learning, and the arts, defence of, against some charges of Rousseau, 438—not the parent of politeness, - - - - *ibid. et seq.*

Lemington-Priors, analysis of two mineral springs at, - - - - 174

Luxury, and corrupt manners, not the progeny of science and the arts, - - - 463 *et seq.*

M

Materiality of heat supported, in opposition to experiments supposed to disprove it, - - 603

Meteorological observations, - - 666

Muriate of Lime (liquid), vapour from it conformable to the same law of force as aqueous vapour, - 579

N

Negro, remarkable change of colour in one, - 314

Nervous Systems of different animals, observations on, 475, *et seq.*—on original defects in the nervous system of the human species, 492—their influence on sensation and voluntary motion, - - 507

O

Oak (Iron or Turkey), a profitable and ornamental tree, - - - - - 167

Oxygenated muriate of potash, experiments on, - 221

Oxygenated muriatic acid, new method of bleaching with, - - - - - 298

P

Party-prejudice, cursory remarks on, - - 1

PAGE.

Pink (colour), appears to some people the same as *sky-blue*, - - - - 32

Politeness, the offspring of woman, not of learning and the arts, 451—not justly chargeable with insincerity and dishonesty, - - - 458

Poplar, Athenian, recommended for beauty and quick growth, - - - - 165

PRIESTLEY'S, Dr. analysis of atmospheric air, remarks on, - - - - - 123

R

Rain, mean annual quantity of, in several *parts* of England, *348—general mean for* England and Wales, 349—*rain* and *dew* equal to the evaporation, and to the supply of springs and rivers, 365—mean quantity of at Manchester, for 8 years, 668—at Kendal for 14 years, - - - - 669

Repulsion of the particles of elastic fluids inferred from experiments to be directly as their temperature, reckoned from the point of total privation of heat, - 601

Ring, antique, found near Castle-field, Manchester, account of, - - - - 533

Rivers of England and Wales, estimate of the quantity of water that flows down them annually, - 353

RUPP, Mr. on Dr. PRIESTLEY'S analysis of atmospheric air, 123—on a new method of bleaching with the oxygenated muriatic acid, - - - 298

S

SMITH'S, Dr. theory of compound sounds defended, - - - - - 653 *et seq.*

Sounding bodies, how we judge of their directions, 622

Sounds (compound), theory of, - - 653

Spirits (ardent), on the distillation of, - - 243

PAGE.

Springs, origin of, 367—are supplied by rain, - 371

Statues, Grecian, present the most perfect beauty of form, - - - - 428 *et seq.*

Steam, see *Vapour*.

Steel and Iron, observations on, - - 109

Thermometer mercurial, supposed not to be an accurate measure of heat, 602—observations on it at Manchester, - - - 667

Timber trees, on three kinds, useful and ornamental, 163

Tragedy and *tragical representations*, essay on, - 319

U

Universal character (in writing, &c.) on an, - 89

UVEDALE'S, Mr. enquiry into the name of the founder of HULN ABBEY, 46—remarks on Dr. FERRIAR'S account of the monument in the church of HULN ABBEY, - - - - 57

V

Vapour, from water in *vacuo*, table of its force determined experimentally from 32° to 212°, and thence inferred from—40° to 325°, 559—from ether of any temperature the same force as that from water of 110° higher temperature, 566 *et seq.*—from other liquids follows the same law of force as the aqueous, 568, *et seq.*—from liquids, is of the same force in air of any density as in *vacuo*, in the same temperature, 572—hence is inferred to be a distinct elastic fluid, - 573

Ventriloquism, attempt to explain it, - 658 *et seq.*

VESCY (family), pedigree of, - - 57

Voices, on the variety of, - - - 58

W

WALKER, Mr. on tragedy and the interest in tragical representations, 319—his defence of learning and

PAGE.
the arts against some charges of Rousseau, - 438

Water, its density greatest at $42\frac{1}{2}°$, 374—expands from that point equally with heat or cold, 375 *et seq.* —may be cooled down to 5 or 6° of Fahrenheit without being frozen, if inclosed in the bulb of a thermometer tube, 376—in a glass may differ 50° in temperature at half an inch in depth, - - 385

Waters mineral, analysis of two, - - 174

WHITE, Mr. on three kinds of timber-trees, useful and ornamental - - - - 163

Winds, observations on them at Manchester, - 669

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